Wishful Thinking or Effective Threat? Tightening Bank Resolution Regimes and Bank Risk-Taking

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Abstract

We propose a framework for testing the effects of changes in bank resolution regimes on bank behavior, particularly on a variety of risk- and business model-measures. By exploiting the differential relevance of recent changes in U.S. bank resolution law (i.e. the introduction of the Orderly Liquidation Authority, OLA) for different types of banks, we are able to simulate a quasi-natural experiment to test otherwise endogenous effects in a difference-in-difference framework. To the best of our knowledge, this identification strategy is unique in its application to regulatory changes in bank resolution. To test our hypotheses, we use a three level dataset: Holding aggregates, bank level data, and loan level data. We find banks that are more affected by the introduction of the OLA to significantly decrease their overall risk-taking and to shift their business model and new loan origination towards lower risk - indicating the overall effectiveness of the regime change. This effect, however, does not hold for the largest and most systemically important institutions, indicating that the application of the OLA does not represent a credible threat to these institutions, leaving the too-big-to-fail problem unresolved. Finally, we find no evidence of gambling between the announcement and enactment of the OLA, presumably since the legislation was passed comparably quickly. Our results intend to contribute to the emerging literature evaluating implications of new regulatory policies, and allow relevant conclusions for the design of bank resolution law, e.g. in the context of the European Banking Union.

JEL classification: G21, G28, G33

Keywords: Bank resolution, bank insolvency, Orderly Liquidation Authority, FDIC, bank behavior, risk-taking

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Prelude

On June 30, 2010, bank resolution law - under which the Federal Deposit Insurance Corporation (FDIC) was able to close any insured depository institution in the U.S. - was applicable to about 10.9% of the Goldman Sachs Group's subsidiaries. At the end of the next reporting quarter, the FDIC had been enabled by the U.S. Congress to eventually resolve 100% of Goldman Sachs Group or any Financial Holding Company according to an extension in bank insolvency law termed Orderly Liquidation Authority (OLA) that had been introduced as part of the Dodd-Frank Act (DFA).

At the time, the Financial Times applauded that "the Dodd-Frank bill makes important strides in ending government guarantees [...] and disincentivising risky behaviour. [...] In place of government bail-outs (like AIG) and painful bankruptcies (like Lehman Brothers) comes a new 'Orderly Liquidation Authority"'.[1] *And the Economist concluded that "the most important provision [of Dodd-Frank] is the resolution authority under which federal regulators can seize any financial company [...]. This is an improvement on the status quo."*[2] *Have these expectations come true? Did such a dramatic change in bank resolution powers influence bank behavior, risk-taking, and business model choices? Do banks operate differently when the threat of being resolved and liquidated by the regulator becomes more realistic in legal, operational, and financial terms?*

1 Introduction

When governments were confronted with seriously distressed banks during the global financial crisis of 2008 and the subsequent European sovereign debt crisis, existing resolution tools proved mostly inappropriate, either because they did not take into account distinctive features of banks or authorities lacked to some extent empowerment, financial resources, and cross-border cooperation to effectively resolve failed banks. A comparison of the failure resolution of Lehman Brothers and Washington Mutual in September 2008 illustrates the importance of effective and appropriate bank resolution mechanisms.[3] Following these recent crisis experiences, bank regulators and legislators have discussed and brought into force significant changes to bank resolution regimes[4] in an effort to improve future bank failure resolution and ultimately to prevent future crises (e.g. Dodd-Frank Act in 2010, German Bank Restructuring Act in 2011, and Financial Stability Board in 2011).

Effective and enforceable bank resolution mechanisms are not only of vital importance to deal with failing banks and minimize costs associated with bank failures. Beyond that, they can have a disciplining effect and thus reduce the probability of bank failure ex ante. Bagehot (1873) already pointed to moral hazard and excessive risk-taking induced by banks' expectation for bailout. Although various rationales for bailout policies can be formulated (e.g. Acharya and Yorulmazer (2007); Diamond and Dybvig (1983); Diamond and Rajan (2005)), several recent studies provide empirical evidence for

[1] See Financial Times, July 12, 2010.

[2] See The Economist, July 3, 2010.

[3] When Lehman Brothers filed for Chapter 11 bankruptcy protection on September 15, 2008, the bankruptcy filing constituted a default action in derivative contracts, leading to massive terminations of derivative positions. As Lehman Brothers was not allowed to provide liquidity to its subsidiaries, its foreign legal entities entered bankruptcy proceedings as well. At the time of Lehman Brother's failure, Washington Mutual experienced a bank run and was put into Federal Deposit Insurance Corporation (FDIC) receivership by its regulator, the Offce of Thrift Supervision (OTS), on September 25, 2008. FDIC sold Washington Mutual's assets, deposit liabilities and secured debt immediately to JPMorgan Chase and the remaining holding company filed for bankruptcy protection the next day. Although Washington Mutual's business had been materially different from Lehman Brothers', its banking business continued to operate without major interruptions, unlike the failure of Lehman Brothers. FDIC (2011) provides an extensive discussion of the differences between Lehman Brother's bankruptcy under Chapter 11 and a hypothetical resolution under a special bank resolution regime, i.e. the Orderly Liquidation Authority.

[4] We use the term 'bank resolution regime' in a wide meaning, not just refering to the actual legal provisions, but also to the (financial or operational) empowerment of resolution authorities. Also, with regard to affected institutions, we do not just refer to banks in their form as insured deposit-taking intermediaries, but to financial institutions with bank features in general (e.g. financial or bank holding companies).

the moral hazard effect of bailout (expectations) on risk-taking, e.g. Black and Hazelwood (2012); Dam and Koetter (2012); Duchin and Sosyura (2012). Reversely, when bailout guarantees cease to be implicit through a credible and enforceable improvement in bank resolution regimes, we expect banks to change their behavior towards less risk-taking and lower probability of distress. This hypothesis is proposed in a recent model of DeYoung et al. (2013), which suggests that a credible improvement in resolution regimes can increase overall bank discipline. We take this as a theoretical foundation for our empirical evaluation.

The introduction of the Orderly Liquidation Authority provides an ideal setup to study this disciplining effect on bank behavior. The OLA has been established through the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 (DFA) and enables the FDIC to seize control and liquidate any financial institution in distress through its administrative resolution regime. Before DFA's enactment, the FDIC's resolution authority only comprised insured depository institutions. With the OLA, FDIC's authority has been extended to institutions that were previously exempted from any specific bank resolution regime, namely Bank Holding Companies (BHCs) and non-bank financial companies. In this paper, we distinguish between BHCs with large nonbank financial asset holdings on the one hand and BHCs with mainly depository bank holdings and independent banks on the other hand. By exploiting the differential relevance of the OLA to these groups, we are able to simulate a quasi-natural experiment that allows us to test otherwise endogenous effects in a difference-in-difference framework.

We address a series of important and novel questions in this paper: Do banks change their behavior when bailout expectations vanish and the threat of being resolved in case of failure becomes more realistic? More precisely: Is the OLA a credible and effective improvement to the resolution regime leading to a reduction in return volatility, asset risk, and default probability of affected institutions? Do banks adjust their business models following the OLA, e.g. with regard to their securities investments, trading acitivities, or funding structure? Is there a change in risk-taking when it comes to new business, more specifically do banks approve and originate less risky mortgage loans? Is the improvement in the resolution regime effective for all banks, and is the resolution threat credible and effective even for banks that are deemed 'too-big-to-fail'? Finally: Can we observe a reverse effect between announcement and enactment of the resolution policy change, which would correspond to theories on gambling (compare e.g. Fischer et al. (2012); Murdock et al. (2000))?

These questions are addressed using a three level dataset: Holding aggregates, bank level data, and loan level data. We find banks that are more affected by the introduction of the Orderly Liquidation Authority to significantly decrease their overall risk-taking after the OLA becomes effective relative to the control group of non-affected banks. On a more detailed level, we find that affected banks also shift their business model and new loan origination towards lower risk. Our results indicate the overall effectiveness of the regime change, which can indeed be interpreted as an improvement in available resolution technology. However, the overall effect does not hold for the largest and most systemically important institutions, indicating that the application of the OLA does not represent a credible threat to these institutions. Hence, even the introduction of the OLA seems to leave the 'too-big-to-fail' problem unresolved - at least for the very largest banks. Finally, we find no evidence of gambling around the announcement and enactment of the OLA, presumably since the legislation was passed and enacted comparably quickly.

We focus our analysis on the U.S. due to the unique identification opportunity and due to data availability, but our results have wider implications. They are not only of concern in evaluating the effectiveness of resolution policy change in the U.S., but can also contribute to regulatory discussions in the context of, for example, an EU-wide joint bank recovery and resolution policy framework that is proposed as part of the planned European Banking Union (European Commission, 2012).

Our paper is intended to contribute to the latest literature on the effects of regulatory actions on bank behavior, particularly risk-taking, e.g. Berger et al. (2012); Black and Hazelwood (2012); Dam and Koetter (2012); Duchin and Sosyura (2012). While these papers mainly focus on the effects of government bailout policies, we investigate the effects of an ex ante disciplining regulatory approach. Although an economic rationale for such disciplining resolution policies has been modeled before (Acharya, 2009; Acharya and Yorulmazer, 2008; Perotti and Suarez, 2002), empirical evidence is limited to the (non-)application of resolution rules by regulators (Brown and Dinç, 2011; Kasa and Spiegel, 2008). A vital implication of resolution regimes, however, has so far mostly gone unevaluated: the effects of their tightening on bank behavior. Therefore, this paper is intended to provide an empirical test of the credibility and effectiveness of changes in resolution regimes with regard to their implications for bank behavior. As a methodological contribution, we propose an identification setup that is - to the best of our knowledge - novel to testing the effects of changes in resolution regimes.

The remainder of this paper is organized as follows. Section 2 gives an overview of the related theoretical literature and core findings of existing empirical research. Our key hypotheses are proposed against this background. In Section 3, we introduce our identification strategy and present initial indicative evidence. Our full model and dataset is described in Section 4. Section 5 presents the results of the analysis, several extensions, and is complemented with robustness tests. Section 6 concludes and provides policy implications.

2 Background, related literature and key hypotheses

2.1 How regulation drives bank risk-taking

Financial economics literature has identified several determinants for bank risk-taking, among them the degree of competition, the degree of information transparency on bank risks, and ownership structure, but also incentives created by bank regulation and safety nets. In this section, we revisit theoretical and empirical literature to investigate how regulation – and particularly the resolution regime – interacts with bank risk-taking.

In general, the literature has mostly focused on four main forms of bank regulation: deposit insurance, capital regulation, restrictions on bank activities, and resolution of banks. Deposit insurance schemes are often described as safety nets against bank runs. However, deposit insurance at a fixed rate (independent of the risk of banks' assets) creates a moral hazard problem, as banks can borrow funds cheaply through insured deposits and invest them in risky assets (Kareken and Wallace, 1978; Merton, 1977). Moreover, insured depositors have little incentive to monitor the bank.[5] This moral hazard problem can be mitigated by making the deposit insurance explicit and leaving some creditors uninsured (Calomiris, 1999; Gropp and Vesala, 2004). Other design features of deposit insurance such as funding, premium structure or membership requirements can also alleviate the moral hazard problem (Barth et al., 2004).

The purpose of capital regulations is to reduce banks' – more precisely bank owners' – risk-taking incentives through forcing them to leave some of their capital at risk as a buffer for future losses. However, a simple capital-to-asset ratio provides incentives to shift to riskier asset portfolios, thus increasing risk-taking behavior (Koehn and Santomero, 1980). A risk-based capital ratio that accounts for asset quality can reduce this asset-substitution problem (Kim and Santomero, 1988; Repullo, 2004). Yet, several further theories suggest negative effects of capital regulation on bank behavior.[6]

[5] Empirical cross-country studies strongly confirm the moral hazard incentive of deposit insurance.Demirgüç-Kunt and Detragiache (2002) show that the existence of deposit insurance increases the probability of banking crises and that this effect is even stronger the more coverage the deposit insurance provides. Demirgüç-Kunt and Huizinga (2004) provide evidence for the adverse effect of explicit deposit insurance on market discipline.

[6] Capital regulations might actually increase bank risk-taking (Besanko and Kanatas, 1996; Blum, 1999; Murdock et al.,

Similar to capital regulation, restrictions on bank activities also aim at more prudent risk behavior by restraining banks from engaging in other risky businesses outside their original activities (Boyd et al., 1998). Empirical studies provide mixed evidence on the risk mitigating effect of activity restrictions (e.g. Barth et al. (2004)).

Resolution of distressed banks is probably the most intricate regulatory area regarding risk-taking incentives. Overall, there are two (opposing) regulatory approaches to handling a distressed bank: bailing out the bank in order to preserve it as a going concern and resolving the bank either through acquisition by another financial institution (i.e. purchase and assumption) or straightforward closure and liquidation. One line of theory predicts that the expectation of being bailed out increases banks' moral hazard as creditors anticipate loss protection in case of bank failure and have little incentives to monitor the bank (or to adjust risk premiums as indicated in Sironi (2003) and Gropp et al. (2006)). A different theoretical approach suggests that bailout guarantees can increase charter values (i.e. through lower funding cost) and hence decrease incentives for excessive risk-taking as banks fear losing these charter values (Keeley, 1990). Connecting both theories, Cordella and Yeyati (2003) and Hakenes and Schnabel (2010) develop models where the positive charter value effect can actually outweigh the negative moral hazard effect and thus lead to more prudent risk-taking behavior of banks protected through bailout guarantees. However, their models depend on specific economic circumstances, banking sector characteristics and/or bailout policy designs. Empirical evidence tends to support the view that bailout policies rather increase than decrease bank risk-taking and moral hazard in the long run.[7]

A credible resolution threat of closing or selling banks in case of failure should decrease excessive risk-taking incentives ex ante. Theoretical models however predict certain caveats: According to Davies and McManus (1991), the effect of the closure threat on bank risk-taking depends on the bank's 'healthiness' (i.e. capital base) and the regulator's closure rule (i.e. specifying closure at a certain capital level). Mailath and Mester (1994) model a time-inconsistency problem where the regulator's bank closure decisions interact with banks' asset choices, leaving the regulator unable to credibly commit to closure policies. Apart from ex ante incentives, closing or selling banks in case of failure can also impact ex post incentives of surviving banks. Perotti and Suarez (2002) consider a model where the acquisition of failed banks enhances charter values of surviving banks (i.e. through greater market concentration) and thus increases surviving banks' incentives for prudent risk behavior. Another conceivable implication on bank behavior could be 'gambling for resurrection'. As theoretically shown in Murdock et al. (2000), bank's incentive to gamble increase, when they lose their charter values. The withdrawal of an (implicit) bailout guarantee due to an introduction of a credible resolution threat can imply higher funding cost and thus a loss in charter values. Hence, banks might start gambling as a reaction to a change in resolution policy.

Taken together, the existing literature proposes, models, and evaluates several effects of bank failure resolution (bailout or closure) on bank behavior. To the best of our knowledge, however, there has not been any study so far that empirically investigates the effects of tightening resolution regimes on bank risk-taking.

2000) and decrease lending activity (Thakor, 1996). Moreover, the recent financial crisis revealed the shortcomings of risk-based capital regulation: Neither did they score well on predicting failure (Berger and Bouwman, 2012; Blundell-Wignall and Atkinson, 2010), nor did they prevent regulatory arbitrage. Rather, when the risk of certain assets is not properly estimated by a regulator, banks have strong incentives to acquire and hoard these assets, thus increasing systemic risk (Acharya et al., 2013).

[7] Black and Hazelwood (2012)and Duchin and Sosyura (2012) provide evidence that (at least large) TARP-funded U.S. banks increased risk-taking after the capital injection. Dam and Koetter (2012) exploit a dataset on capital injections in Germany and find that bailout expectations (through observed capital injections) increase risk-taking in the whole banking sector (measured as probability of default). However, using the same dataset, Berger et al. (2012) show that banks receiving capital injections decrease risk-taking (measured as ratio of risk-weighted assets to total assets). The results in Gropp et al. (2011) are also mixed: They find no evidence for increased risk-taking by banks protected by bailout guarantees.

2.2 A theoretical model of bank closure

In the theoretical literature, bank resolution regimes have attracted more and more interest over recent years. One of the most comprehensive theoretical models of the interaction between resolution law, its credibility and application, and bank behavior was recently offered by DeYoung et al. (2013). Building on the time-inconsistency problem of bank closure decisions formulated by Mailath and Mester (1994) and Acharya and Yorulmazer (2007), the authors model the regulatory closure of a bank as a trade-off between short-term liquidity and long-term discipline. The model assumes banks that are inherently fragile and suffer from moral hazard with regard to excessive risk, complexity, and volatility. Essentially, there are two alternatives for the regulator to deal with this. On the one hand, banks can be disciplined by a strict closure and resolution policy in case of failure. Unfortunately, this discipline only materializes in the long run. On the other hand, while they help to establish discipline, available resolution technologies usually suffer from limitations. These limitations, such as slow processes, missing information, or legal limits to available regulatory instruments, might (temporarily) lead to illiquidity in the case of bank closures. This might result in a detrimental impact on the economy as a whole (e.g. Ashcraft (2005)). Hence, the regulator - despite knowing about the long run benefits of discipline - also has an intrinsic motivation to prefer bailouts or forbearance over straightforward closure.

DeYoung et al. (2013) model the outcome of this trade-off as determined by two parameters. The first one is the time discount rate of the regulator - the higher it is, the stronger is the regulator's preference for liquidity, i.e. bailout. Effectively, this discount rate proxies for the pressure for immediacy that regulators and economic policy makers are experiencing, e.g. political pressure to preserve liquidity during a crisis.[8] The resolution technology available to the regulator is the second parameter determining the trade-off. The better this technology is, the faster and more efficient a bank closure can be executed, the more liquidity is preserved. Consequently, regulators with better resolution technologies at hand are - under the assumption of equal time discount rate - more induced to enforce discipline, i.e. closure.

This model provides several testable implications. First, improvements in resolution technology, such as legal changes or operational empowerment of the regulator, make a regulatory policy preferring discipline (i.e. closure in case of failure) more likely. If the technological improvement is known and credible to banks, they will act rationally by adjusting their behavior towards more discipline ex ante. Hence, an improvement in resolution technology should induce less excessive risk-taking and the adoption of more conservative business models, ceteris paribus. Second, this outcome depends on the credibility of the application of the new resolution technology. The new policy instruments will only be effective when complemented by political will, i.e. a low time discount rate that increases the willingness of regulators to accept potential short-term illiquidity following bank resolution for long-term gains in discipline. Using these general implications as our theoretical foundation, we test whether the change in specific resolution technologies is indeed an effective and credible improvement that alters the behavior of affected banks.

2.3 Hypotheses on the effects of tightening resolution regimes

Building on the theory of bank resolution and previous findings discussed above, we yield the following hypotheses and subject them to econometric testing.

[8] Several empirical studies confirm the tendency for bailout and forbearance in times of macroeconomic or systemic stress. Brown and Dinç (2011) and Kasa and Spiegel (2008), for example, find that regulators are less likely to close a bank if the whole banking system is in a crisis.

Main hypothesis: If the a change in bank resolution regimes (e.g. in the legal provisions governing bank resolution) indeed represents a credible and effective improvement to bank resolution technology, it will change the behavior of those financial institutions affected towards less risk-taking and safer business models. We thus expect a decrease in risk measures for affected banks after the change becomes effective.

Extended hypothesis I: The above effect might vary with the credibility and the political will to truly resolve failed institutions. Both credibility and political will can be influenced and hence proxied by exogenous (e.g. elections, overall state of the economy) or endogenous (e.g. characteristics of the bank such as systemic importance that influence the discipline-liquidity trade-off) variables. If the application of the new regime is not credible due to bank-specific characteristics, we expect to find a lower or even no effect on the respective banks' risk-taking.

Extended hypothesis II: Changes in bank regulation that reduce banks' charter value might lead to gambling, particularly during the time after public announcement and before legal enactment. If the political and legislative procedures around the introduction of changes in bank resolution regimes provide opportunities for gambling, we expect to see an increase in risk measures for the affected banks after announcement and before effective enactment of the change.

3 Identification strategy - An application to changes in the U.S. bank resolution regime

While the existing literature and the theoretical model of DeYoung et al. (2013) provide testable implications of changes in resolution regimes, the actual empirical testing is challenging due to the endogenous relation between bank behavior and resolution. In order to overcome these endogeneity concerns and to test our hypotheses formulated above, we apply the theory of failed bank resolution to a specific change in the U.S. bank resolution regime, the introduction of the Orderly Liquidation Authority. We argue that the circumstances of the OLA introduction resemble a natural experiment setup that can be exploited using a difference-in-difference model. This section describes the fit of this specific resolution regime change and the identification strategy by (1) discussing whether the OLA indeed constitutes an improvement in resolution technology (i.e. whether it can indeed be taken as a relevant treatment), (2) timing the introduction of the OLA (i.e. the treatment effect), (3) defining differentially affected financial institutions (i.e. treatment and control group). Finally, we present some initial evidence that supports our identification setup and merits the more formal evaluation in the sections to follow.

3.1 Identifying the treatment - Is the Orderly Liquidation Authority an improvement in resolution technology?

When the financial crisis hit in 2008 (and surely before), U.S. bank resolution law suffered from two significant shortcomings. We will argue that the Orderly Liquidation Authority represents a significant technological improvement on these two issues.

As a first issue, financial institutions in the U.S. were subject to two different insolvency and resolution regimes. One pillar of bank insolvency legislation was the Federal Deposit Insurance Act (FDIA), that covered all insured depository institutions, particularly commercial banks, thrifts, and savings banks holding a national or state charter. The FDIA stipulates a special resolution regime for these institutions - an administrative insolvency procedure. The existence of this special bank resolution regime stems from the conviction that banks are somewhat distinctive, particularly with regard to insolvency. Marin and Vlahu (2011) provide a detailed analysis of the characteristics of banks that advocate a special resolution regime - among the most important ones are (1) the inherent instability of banking and the threat of runs, (2) particularly negative externalities of bank failures, and (3) the potential for moral hazard due to deposit insurance schemes or implicit guarantees. While the corporate insolvency law does not cover these aspects explicitly, the FDIA regime takes the special role and functioning of financial institutions into account. It is designed to allow timely intervention and resolution of insolvent banks while limiting moral hazard as well as potentially detrimental effects to liquidity, sound banks, and the real economy. In order to achieve the goal of a least cost (and least adverse effects) resolution, the special resolution regime deviates significantly from the regular, judicial insolvency procedure with regard to insolvency triggers and initiation conditions, resolution instruments, financing, and possibilities for appeal and review (Bliss and Kaufman, 2006; Marin and Vlahu, 2011). Under these provisions, the Federal Deposit Insurance Corporation (FDIC) has powers to promptly intervene upon certain initiating conditions, such as critical undercapitalization, without having to wait for the filing of a default event or for court decision. In this case, the license of the bank can be revoked by its primary regulator and the FDIC can be determined as the conservator or receiver, ousting management and shareholders, taking over the bank, and ultimately preparing it for purchase and assumption by another financial institution or for closure and liquidation. In order to preserve liquidity, charter value, and operations of the bank, the FDIC typically intervenes overnight or over the weekend and is able to pay off all insured depositors - if need should be - from the Deposit Insurance Fund previously collected from insured institutions (Bliss and Kaufman, 2006; DeYoung et al., 2013).

While the FDIA covers insured depository institutions under national and state bank charters, the FDIC did not have legal powers for intervention when it comes to the failure of bank holding companies, financial holding companies, or other non-bank financial institutions. Instead, the default legal provisions of corporate insolvency law, i.e. the insolvency procedures according to Chapter 7 and Chapter 11 of the U.S. Federal Bankruptcy Code, applied. These procedures typically protect the owners from creditors, take long time periods for resolution during which funds for depositors and borrowers might not be available, and require a restructuring plan as a precondition before making decisions on larger asset sales (DeYoung et al., 2013). Since the financial holdings and non-bank financial institutions in question - among them several of the institutions that have been identified as systemically important - exhibit similar characteristics to banks as described by Marin and Vlahu (2011), an application of these corporate insolvency procedures might cause severe disruptions.[9] While these institutions were effectively exempted from the special bank resolution regime, the default corporate law was apparently inappropriate to efficiently resolve their insolvency. Hence, this was widely considered as a major deficiency in the resolution regime for financial holdings and non-bank financial firms, which might have even protected these institutions from actual failure by making bailout the only available choice (FDIC, 2011; Marin and Vlahu, 2011).

Moreover, even if the FDIC had been legally empowered to apply its resolution procedure to non-bank financial institutions, there would have been a financial limit as to which institutions it could have effectively taken over: While the Deposit Insurance Fund amounted to a record high of USD 52.4 billion at the onset of the financial crisis, the deposits of Bank of America alone were about 10 times larger than this (albeit not all insured). The sheer order of magnitude of this difference illustrates the second significant issue gripping the resolution technology available to U.S. regulators before 2010: Not just incomprehensive legal provisions, but also the insufficient financial endowment of the regulator

[9] In fact, several studies examine the inapplicability of corporate insolvency law to financial institutions, e.g. refering to one of the few bankruptcy cases of financial firms: Lehman Brothers Holding Inc. (FDIC, 2011).

prevented an effective application of bank resolution and made bailout the regulator's preferred choice in most cases for financial holdings and non-bank financial companies.[10]

Recognizing the need for alterations in bank resolution law and for stepping-up operational and financial capabilities of the regulator, U.S. federal legislators passed the Orderly Liquidation Authority as part of the wider financial sector reform package, the Dodd-Frank Act (DFA, Title II). The new provisions stipulated by the OLA can be considered as an improvement to resolution technology in several dimensions. First, it extends a special insolvency and resolution regime to financial institutions previously uncovered by bank resolution law. More specifically, it stipulates that any firm determined as a covered financial company according to Sec. 201 and 203 of the DFA can be put into an administrative insolvency and resolution procedure. Effectively, this provision covers any financial institution in the United States.[11] The determination of a financial institution as a covered financial company is made by the Secretary of the Treasury, following the vote of the FED board and FDIC board, and in consultation with the President. It initiates the orderly liquidation procedure, with only limited judicial appeal ex ante.[12] Technically, this procedure is very similar to the existing FDIA regime, with the FDIC being appointed as receiver of the financial company. Once under receivership, the FDIC is empowered to close and liquidate the firm, to pursue a purchase and assumption resolution, or to set up a bridge financial institution. These resolution instruments also resemble the FDIA regime insofar as they cause losses to shareholders and unsecured creditors, replace the management, and protect liquidity in a way that is superior to regular insolvency law.

Second, Title II of the DFA sets up a new Orderly Liquidation Fund that also financially enables the FDIC to act as the receiver and pursue the orderly liquidation of covered financial companies. While the fund is set up in the Treasury, the FDIC is authorized to borrow from it for covering the cost of orderly liquidation and administrative expenses. Moreover, the FDIC is empowered to charge ex post risk-based assessments to financial companies[13] in order to repay the Orderly Liquidation Fund (DFA, Title II, Sec. 210).

Taken together, the Orderly Liquidation Authority can be interpreted as an improvement to resolution technology (in the sense of DeYoung et al. (2013)) in at least two dimensions. First, we interpret the OLA as an improvement in terms of legal authorities as it alleviates the previous limitation of the FDIC to only place a certain group of financial institutions into a special bank resolution procedure. Rather than focusing only on insured depository institutions, the special resolution regime is now extended to other financial companies as well. Second, the establishment of the Orderly Liquidation Fund significantly improves the financial and operational capacity of the FDIC to effectively act as receiver and liquidity guarantor. There is now less reason to prefer bailout over resolution when large financial institutions fail - at least theoretically. These improvements might not establish an optimal and ultimate resolution regime - rather, there is a broad discussion in the literature suggesting changes

[10] It should be noted that bailout was not preferred for a myriad of smaller banks that were covered by the FDIA and for which the Deposit Insurance Fund proved large enough: Between 2008 und 2010, the FDIC resolved the record number of more than 300 banks.

[11] The determination as a covered financial company essentially requires three conditions to be fulfilled. Firstly, the firm in question needs to be a financial company, i.e. a bank holding company, a non-bank financial company supervised by the FED board, or any company predominantly engaged in financial activities. Secondly, it is not an insured depository institution covered by the FDIA regime. Finally, the determination is made provided the existence of all criteria outlined in Sec. 203b, i.e. the firm is in (danger of) default, the resolution according to otherwise applicable legal provisions would have adverse consequences for financial stability, there is no viable private sector alternative, impact on creditors and shareholders is appropriate, all convertible debt has been ordered to be converted, and the OLA is deemed effective (DFA, Title II, Sec 201, 203).

[12] In fact, the board of the determined covered financial company can ask the Secretary of the Treasury to petition for a formal authorization by the U.S. district court in the District of Columbia. This court can order the authorization after finding that the determination as covered financial company is not arbitrary and capricious. If the court does not decide within 24 hours, the authorization is automatically granted by the operation of law (DFA, Title II, Sec. 202).

[13] More specifically, Sec. 210 stipulates that the assessments are to be imposed on large non-bank financial institutions, precisely bank holding companies with consolidated assets exceeding USD 50 billion and non-bank financial companies supervised by the FED board.

that might be even more appropriate (Bliss and Kaufman, 2011; Edwards, 2011; Fitzpatrick et al., 2012; Scott et al., 2010; Scott and Taylor, 2012; Zaring, 2010). However, most of these commentators (and the leading financial press quoted in the prelude of this paper) agree that the Orderly Liquidation Authority at least represents a theoretical improvement to the pre-existing regime. In fact, DeYoung et al. (2013) themselves describe it as a 'positive technological shock for U.S. bank regulators' and add the prediction that (if effective) this will make resolution of insolvent financial institutions more likely and hence reduce their incentives to choose high-risk business strategies.

Hence, we argue that the introduction of the OLA is indeed a significant improvement to resolution technology and will use it as the treatment whose effect we test going forward.

3.2 Timing the treatment - When did the treatment take place?

As with any legislative process, the introduction of the OLA stretched over a significant timespan from the generation of the idea to the bill being passed and signed into law by the President. The earliest proposal for legislation regarding an Orderly Liquidation Authority was contained in the financial sector reform package suggested by the Obama administration in June 2009 (Department of the Treasury, 2009). A revised proposal for the Orderly Liquidation Authority was announced as part of the reform package that was later named the Dodd-Frank Act in December 2009. The major legislative process took place over the following six months in the House of Representatives and the Senate. Finally, the Dodd-Frank Act (and with it the OLA) was passed by the U.S. Congress in July 2010 and signed into law by President Obama on July 21, with immediate effect. For our purposes, the first indication when banks were confronted with the likely change of regulation planned by the Obama administration (June 2009) until the actual enactment of the legislation (July 2010) can be understood as the treatment period.

Since our dataset is constructed from quarterly data, we define all periods before and including the second quarter of 2009 as pre-treatment periods and all periods after and including the third quarter 2010 as post-treatment periods.[14]

3.3 Identifying treatment and control group - Where financial institutions differentially affected?

An important pillar of our identification strategy is the differential effect of the OLA on financial institutions that was already indicated above. While insured depository institutions were subject to bank resolution law before, other financial institutions - specifically bank holding companies (BHCs) and non-bank financial companies - were de facto not resolvable in an appropriate manner due to the legal inapplicability of the FDIA and the economic inapplicability of corporate bankruptcy law. Essentially, the introduction of the OLA only affected the latter group by exposing them to a credible threat of resolution for the first time.

However, the actual situation is a bit less clear cut, as most holding companies own bank subsidiaries that fall under the FDIA resolution authority.[15] In some cases, the bank subsidiary even makes up 99% of the holding's assets, with the holding just being a legal mantle used for accounting, tax, and other purposes. In order to not treat the constructs that have 99% of assets regulated by the FDIA and those that only have 10% the same way, we propose an indicator that measures the share of assets of a holding company not subject to the FDIA resolution regulation. In our view, this indicator

[14] Due to data availability and data quality we have to define slightly different pre- and post-treatment periods in the loan level dataset. Refer to the following section for more details.

[15] As indicated in the prelude, even Goldman Sachs Financial Holding owned subsidiaries (such as the Goldman Sachs Bank) that fall under the definition of an insured depository institution and were hence subject to resolution procedures governed by the FDIA.

has the advantages that it captures the essence of our identification idea and is simple to compute. While we can also use the continuous indicator to build an interaction term, we will start with a pure difference-in-difference setup by defining cutoffs that identify treatment and control group. We define all BHCs (and banks belonging to a BHC) that hold more than 30% non-FDIA-regulated assets as particularly 'affected' by the regulatory change, i.e. as treatment group. On the other hand, we define all BHCs (and banks belonging to a BHC) that do not have any or less than 10% non-FDIA-regulated assets as 'not affected', i.e. as control group. However, since these cutoffs are admittedly arbitrary, we test several alternative cutoffs as well as the use of the continuous indicator in our robustness checks.

Taking the differential exposure to FDIA regulation as the criterion for distinguishing treatment and control group enables us to employ a difference-in-difference setup to estimate the effect of OLA on risk-taking. As our key identifying propositions, we have to assume that (1) treatment and control group are developing in parallel (but not necessary at the same level) and (2) only the treatment affected the treatment and control group differently (i.e. what we measuring is actually the treatment effect and not something else). We argue that both is the case and present evidence for (1) in the following sections. Regarding the differential treatment effect (2), we assume that most other changes that took place at the same time as the introduction of the OLA concerned banks independently of their share of assets under FDIA regulation. The first argument supporting this assumption is that among several regulatory changes that took place at that time, the introduction of OLA is regarded the most influential one (see, e.g., the quote from the Economist in the prelude). Secondly, other changes might have been discussed or passed in the context of the Dodd-Frank Act, but many of them only became effective at later dates.[16] And, thirdly, even if other important changes (in regulation or other aspects of the banking business) became effective at the same time, none of those did arguably affect banks differentially depending on their share of FDIA regulated assets. Finally, one might rather argue that BHCs with large unregulated share run a very different business model and are hence (assuming that this cannot be controlled for by covariates and fixed effects, which we will actually do) experiencing a differential effect from other regulatory or financial market changes that took place at the same time. This would, for example, be the case for holdings with large investment banking or trading units, which were a particular target of the regulation that was passed at that time. Succumbing to this line of reasoning, we resort to the bank level (besides using it as a robustness check) where these effects should not be pronounced. Rather, the business models of insured depository banks (the ones that are individual banks or belong to an affected group vs. the ones that belong to a non affected group) should be far more comparable as the business models of a holding with large investment banking departments and a holding where depository banking represents 99% of the assets.

Nevertheless: To the extent that parallel changes might have affected banks' risk-taking proportionally to their non-FDIA-regulated share as well, we would also pick up their effect in our estimates. While we are convinced not to find such effects outside the regulatory reform area, it could admittedly be that, for example, regulatory attention to mostly non-FDIA-regulated institutions increased with the introduction of the new resolution law. Hence, we should be aware that we are not just measuring the effect of a mere change in the law, but in the whole resolution regime, including credibility, capability (e.g. the Orderly Liquidation Fund), and attention of the regulator that this legal change evoked.

3.4 Initial evidence - Does it really make a difference?

Is the OLA a technological improvement that is credible and effective? Is there enough political will to use it? Does this new threat invoke a change in bank behavior, particularly for the most affected

[16] See, for example, the detailed overviews of implementation timelines and effective dates produced by Anand (2011); CCH Attorney-Editor (2010); DavisPolk (2010).

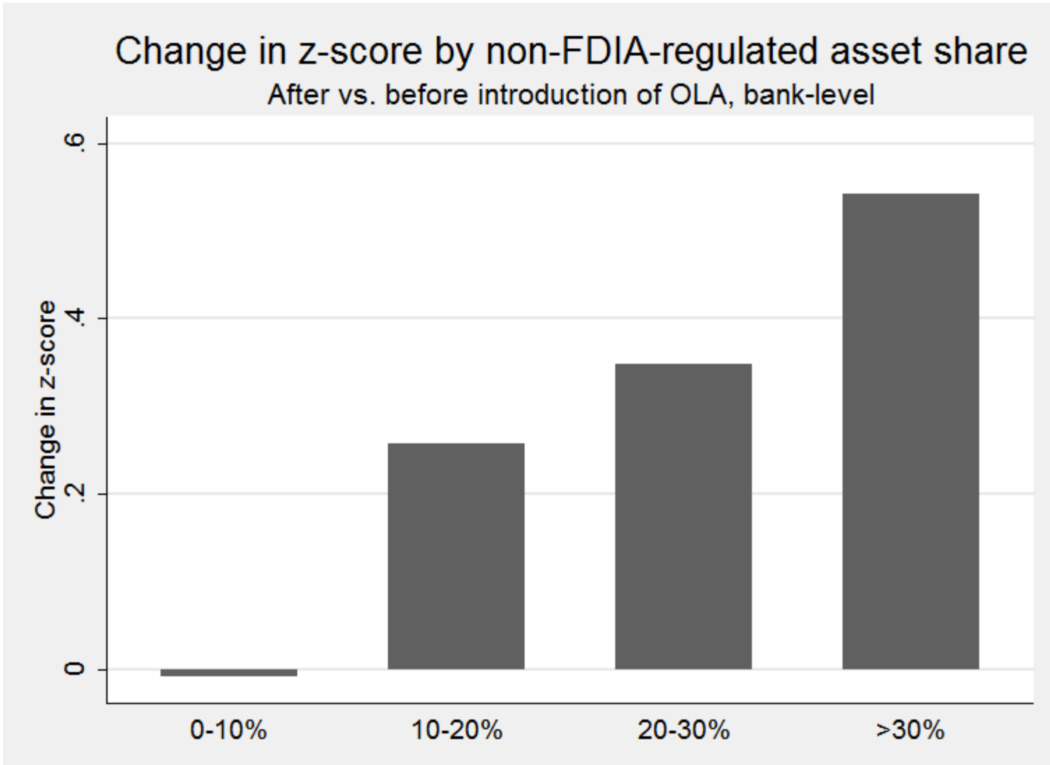

Figure 1: **Change in z-score by non-FDIA-regulated asset share**

institutions, i.e. those ones covered by a special resolution regime for the first time?

Figure 1 provides a first indication that the non-FDIA-regulated share could indeed be related to changes in bank risk-taking after the introduction of the OLA: We plot the average difference in overall bank risk between the pre- and post-treatment over ranges of non-FDIA-regulated share. As a measure for bank risk, we use the average z-score, which is a composite measure approximating the inverse of the default probability, i.e. higher z-scores stand for less overall bank risk.[17] Although this is only a very rough indication, it is interesting to note that higher ranges of non-FDIA-regulated shares correspond to higher increases of the z-score, i.e. lower overall bank risk, after the introduction of the Orderly Liquidation Authority.

Figure 2 and 3 provide an intuition of how affected (i.e. treatment) and non-affected (i.e. control) banks' overall risk develops over a longer time and reacts to the introduction of the Orderly Liquidation Authority. Again, we depict the average z-score of each group as a measure for overall bank risk - this time taking the absolute values and evaluating them over time. Since the z-score incorporates the standard deviation of returns, we have to compute it over a period stretching several quarters. We do this for 8-quarter periods (Figure 2) and 4-quarter periods (Figure 3), both pre- and post-treatment, excluding the treatment period as defined above (Q3 2009 - Q2 2010).

Admittedly, this is only a very crude evaluation that does not control for potentially omitted variables and other sources of endogeneity beyond the bivariate difference-in-difference setup. But several interesting patterns emerge from the two figures. First, the differential behavior of affected and non-affected banks around the treatment is evident: In both figures, the affected banks experience a much stronger increase in the z-score between the pre-treatment and the post-treatment period. However, the key identifying assumption of difference-in-difference is that the two groups would exhibit a parallel development in the absence of treatment. We can test this parallel trend assumption by

[17] Refer to the following section for a detailed description of the composition of the z-score.

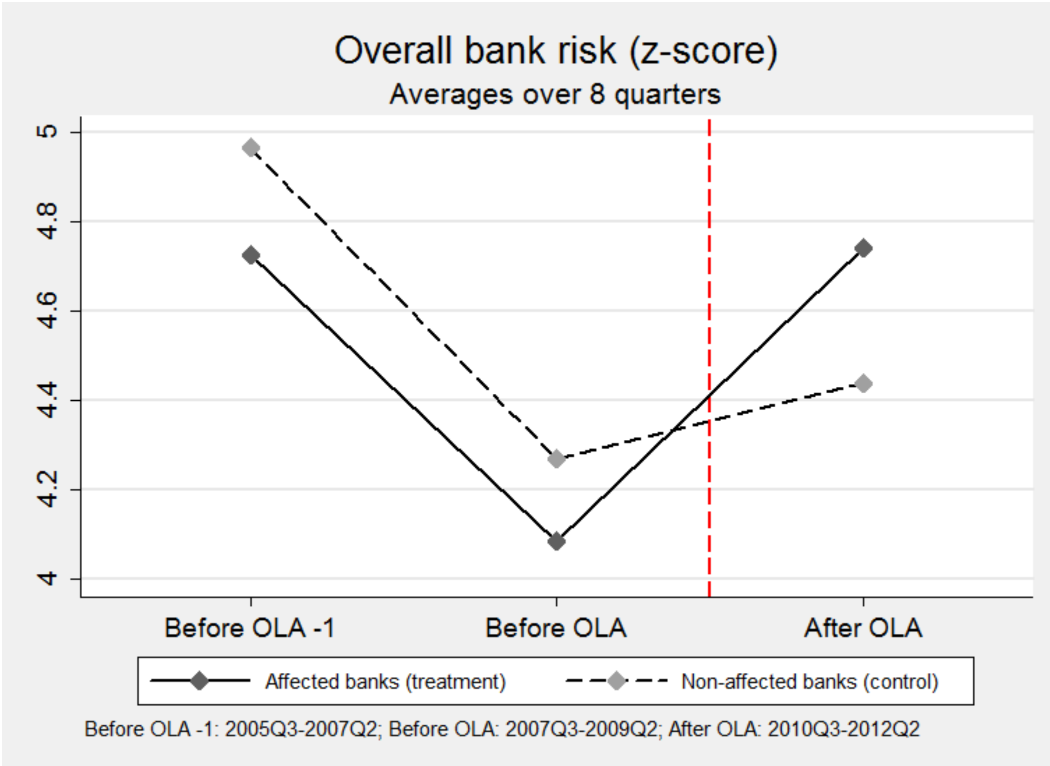

Figure 2: **Bank risk-taking before and after OLA**

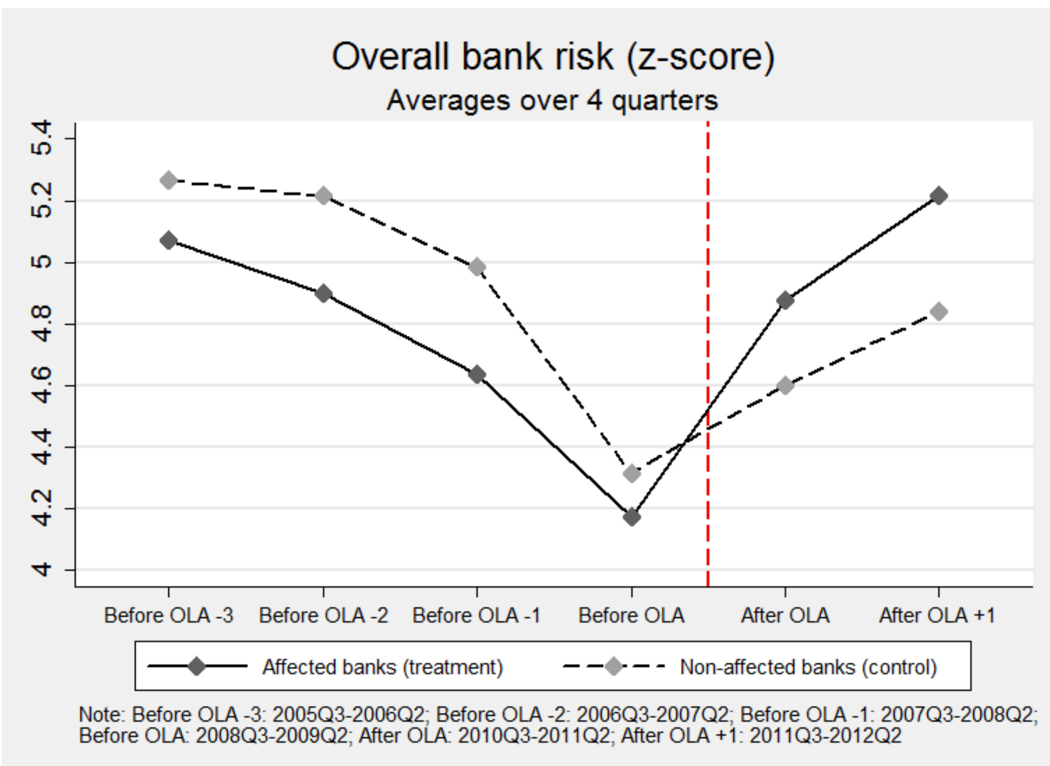

Figure 3: **Bank risk-taking before and after OLA**

including additional periods of data before and after the pre- and post-treatment period. Indeed, we find a parallel trend before the treatment: In both graphs, affected and non-affected institutions develop approximately in parallel in the absence of treatment. Figure 3 even allows us to add an additional period after the post-treatment period, which again exhibits a parallel trend. It is interesting to observe that affected banks consistently exhibit higher risk (lower z-score) before the treatment and reverse this after the treatment. Taken together: In the absence of treatment, both affected and non-affected banks seem to develop in parallel. It is only at the introduction of the OLA, that the treatment group of affected banks experiences a materially different behavior, i.e. a larger decrease in risk-taking, as compared to the control group of non-affected banks. Consequently, these results are a first indication that our main hypothesis might be correct. We test both the main hypothesis as well as the parallel trend assumption in a more rigorous empirical framework below.

4 Model and dataset

4.1 Baseline model

For a more rigorous empirical testing, we construct a difference-in-difference model whose baseline version is depicted in equation 1. The main dependent variable of the model is $Risk_{i,t}$, one of the risk measures outlined below. The core explanatory variables are $afterOLA_t$ indicating before or after treatment (i.e. improvement in resolution technology) and $AFFECTED_i$ being a dummy variable set to 1 for those institutions affected by the improvement in resolution technology and to 0 for the control group (non-affected). Bank (γ_i) and time (δ_t) fixed effects are used to control for influences constant either over time (e.g. time-invariant bank characteristics) or across banks (e.g. state of the economy or the financial system in a specific quarter). The model is complemented by a set of control variables ($X_{i,t}$) to control for additional covariates that might vary by treatment and control group and influence bank behavior. If our main hypothesis holds true, we expect to see a decreasing effect of the difference-in-difference term on risk, expressed in the direction and significance of parameter β_3.

$$\begin{aligned} Risk_{i,t} =& \alpha + \beta_1 * afterOLA_t + \beta_2 * AFFECTED_i \\ &+ \beta_3 * (afterOLA_t * AFFECTED_i) \\ &+ \gamma_i + \delta_t + X_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{1}$$

In order to ensure the robustness of our results, we test our hypotheses on different levels and using alternative empirical setups and datasets. First, we identify bank level data from quarterly call reports that we merge with data from quarterly BHC reports in order to construct a dataset covering financial data on bank- and BHC-level. This dataset enables us to compute and test bank level risk measures as dependent variables in the above setup. Additionally, we define several measures for business model choices (e.g. regarding portfolio decisions or funding structure) that can be tested on the bank level. Second, we investigate risk-taking decisions on the level of new mortgage loan business. Therefore, we construct a loan level dataset using the Home Mortgage Disclosure Act (HMDA) Loan Application Registry.

4.2 BHC and bank level dataset

We construct the bank level dataset based on two main sources. On the individual bank level, we assemble data from the Consolidated Reports of Condition and Income (FFIEC031/041), commonly known as call reports. These reports cover several hundred items of financial data, which any bank with a state or national charter is required to file on a quarterly basis with the FFIEC. We construct a sample that contains the full set of banks and financial data for the period covering the first quarter of 2005 till the second quarter of 2012. In addition, we assemble a second dataset on the Bank Holding Company level. BHCs are required to file quarterly financial reports on a consolidated and parent-only level (FR Y-9C/LP/SP), which are available from the FED Chicago. As for the individual bank data, we construct a sample that contains the full set of BHCs and selected financial data for the period covering the first quarter of 2005 till the second quarter of 2012. In a third step, we obtain identifiers for the top-holders, i.e. the ultimate owner, of any individual bank from the FDIC's Statistics on Depository Institutions (SDI), to match both the individual bank level and the BHC level datasets. This matched dataset enables us to identify and compute all necessary variables as defined below. Panel A of Table 1 provides summary statistics and sources of the data on the BHC level, while Panel B focuses on the bank level data.

Dependent variables: Risk and business model measures In order to conduct a series of robustness checks, we use several measures of risk-taking on the overall bank (or BHC) level. Our primary measure is the z-score of each bank, which is defined as $Z = (\overline{RoA} + CAR)/\sigma RoA$, with $\overline{RoA}$ being the mean return on assets, CAR the capital asset ratio, and σRoA the estimated standard deviation of the return on assets. Mean and standard deviation of return on assets are computed over 8-quarter periods (and additionally over 4-quarter periods for robustness tests). Very few banks for which less than 3 datapoints in one of the periods are available for this computation are removed from the sample. The z-score has been widely used in the empirical literature as a proxy for overall bank risk (e.g. Boyd et al. (2010); Dam and Koetter (2012); Gropp et al. (2010); Laeven and Levine (2009); Roy (1952)). Essentially, it measures the number of standard deviations by which a bank's return on assets would have to fall below its mean in order to deplete the available equity. If we define default as losses exceeding equity, the z-score can be interpreted as a measure for distance to default or the inverse of the default probability (Laeven and Levine, 2009; Roy, 1952). Hence, a higher z-score indicates that a bank is more stable, i.e. is associated with less overall risk. We follow Laeven and Levine (2009) in computing the natural logarithm of the z-score.[18]

In addition, we use the σRoA as an alternative risk measure that focuses exclusively on the volatility of banks' return on assets. The return volatility has been used as a measure for overall bank risk in several empirical studies before (e.g. Dam and Koetter (2012); Laeven and Levine (2009)). We complement the z-score and σRoA with an alternative overall risk measure - average asset risk - which is defined as $RWA/assets$, with RWA being the risk-weighted assets. This measure gives an indication of average asset risk (albeit only in a pre-defined, regulatory sense) and has also been used in the empirical literature (e.g. Berger et al. (2012); De Nicolò et al. (2010)). While the average asset risk is a relatively simple measure and risk weights have been criticized as inadequate expression of true risk, this measure offers the advantage to be computable on an individual quarterly level. In any case, we use alternative risk measures as dependent variables to test the robustness of our results.

In order to test the impact of the regulatory change on the business model of banks, we also define a set of additional dependent variables that proxy for business model choices. With regard to portfolio risk choices, we use some of the measures suggested by Duchin and Sosyura (2012). In detail, these are the trading asset ratio (defined as ratio of assets held in trading accounts to total assets), the low risk securities ratio (defined as the ratio of securities of U.S. government agencies and subdivisions to total securities), and the high risk securities ratio (defined as the ratio of equity securities, asset-backed securities, and trading accounts to total securities). Additionally, we use the CRECD loan ratio, which is defined as the sum of commercial real estate loans (CRE) and construction and development loans (CD) divided by total loans. This ratio is used as a proxy for the degree of complex and risky loans

[18] As the z-score is highly skewed, its natural logarithm is assumed to be approximately normally distributed.

on a bank's balance sheet and has been shown to be associated with risky business models more prone to bank failure (e.g. DeYoung (2013)).

Beyond the asset side, we also take a measure from the liability side of banks' balance sheets into account. More precisely, we test the effect on the deposit funding ratio, which is simply defined as deposits divided by assets. This measure is intended to capture the riskiness of the funding structure and the vulnerability to liquidity shocks.

Finally, we also define a measure for risk in income structure. For this, we use the non-interest income ratio, which we compute as average non-interest income divided by average total income.[19] Non-interest income, particularly from non-core activities such as investment banking, venture capital and trading activities, has been shown to be relatively volatile compared to interest income (DeYoung and Roland, 2001) and to be associated with higher overall bank risk (Brunnermeier et al., 2012; DeJonghe, 2010; Demirgüç-Kunt and Huizinga, 2010).

It should be noted that all of the dependent variables are calculated from accounting data, using the call reports and BHC reports datasets. Despite their known shortcomings, we prefer accounting data over market data as the latter would significantly reduce our sample size, particularly for smaller banks.

Explanatory variables and controls In accordance with the identification strategy and the baseline model outlined above, the treatment dummy $AFFECTED_i$, the treatment-period indicator $afterOLA_t$, and particularly the interaction between the two are defined as our main explanatory variables. In order to identify the affected (i.e. treatment) group, we compute an indicator capturing the non-FDIA-regulated share of total assets of a bank holding company. We do this by summing up the total assets of all insured depository institutions (i.e. the ones that fall under the FDIA-regulation and hence are subject to FDIC resolution authority) and scaling it by the total consolidated assets of the BHC (including the non-bank, non-FDIA-regulated assets). For independent banks (i.e. insured depository institutions that do not belong to a BHC), we set the non-FDIA-regulated share to 0. The dummy indicating affiliation to the treatment group, $AFFECTED_i$, is set to 1 for all BHCs (and banks belonging to a BHC in the bank level dataset) that hold more than 30% non-FDIA-regulated assets, i.e. the group of BHCs and banks that is particularly affected. While the non-FDIA-regulated share of assets varies between 0 and 100%, it is rather skewed towards the lower end, as most holding companies own bank subsidiaries that fall under the FDIA resolution authority, some even exclusively. A cutoff at 30%, however, delivers a sufficiently large treatment group. Moreover, a share of 30% is arguably a significant size of the total business of a bank, which will reasonably influence overall business decisions and consequently affect institutions' behavior. At the lower end, we set $AFFECTED_i$ to 0 for all BHCs (and banks belonging to a BHC) that do not have any or less than 10% non-FDIA-regulated assets. Admittedly, these cutoffs are highly arbitrary. Thus, we do not only use several alternative cutoffs, but also an interaction with the continuous variable of non-FDIA-regulated share of total assets to pursue additional robustness tests.

The second main explanatory variable, $afterOLA_t$, is set to 1 for all periods between the third quarter 2010 and the second quarter 2012. It is set to 0 for the eight quarters preceding the treatment, i.e. from the third quarter 2007 to the second quarter 2009. To be able to formally test the parallel trend assumption, we define a second pre-pre-treatment period stretching over the eight quarters from the third quarter 2005 to the second quarter 2007. As a robustness check, we use a second set of $afterOLA_t$ and all variables referring to it, which defines $afterOLA_t$ over 4 quarters around the treatment period.

In addition to the main explanatory variables, we control for a host of additional covariates that

[19] Note that we average over the 4- or 8-quarter periods defined above in order to balance single quarter effects.

might influence bank risk-taking and business model decisions, and that vary over banks and quarters (i.e. that are not captured by the bank and time fixed effects in our model). In detail, these are total assets as a proxy for bank size, capital ratio (defined as equity capital to total assets), return on assets as a proxy for earnings capability, and liquidity ratio (defined as cash and balances at other depository institutions to total assets). All of these variables are computed from the call report and BHC report datasets. Furthermore, several recent analyses have shown that banks tend to increase risk when they receive bailout assistance from the government, e.g. from the Capital Purchase Program (CPP) as part of the Troubled Asset Relief Program (TARP) (Black and Hazelwood, 2012; Duchin and Sosyura, 2012). We follow these studies and add an indicator for the CPP status of a bank that is 1 if a bank is a current recipient of CPP funds in a given quarter and 0 otherwise. Data for this indicator is obtained from the U.S. Department of the Treasury CPP Transactions Report.

4.3 Loan level dataset

To test our hypotheses on risk-taking concerning new business operations - more specifically new mortgage loan business - we use the HMDA Loan Application Registry as our loan level dataset. HMDA requires most mortgage lenders to collect and report data on all mortgage loan applications on an annual basis. According to Dell'Ariccia et al. (2012), the HMDA dataset comprises approximately 90% of all U.S. mortgage loan applications. The HMDA dataset is a comprehensive registry containing loan information (e.g. loan purpose and loan amount), applicant information (e.g. race and gross annual income), infomation on status of the loan application (e.g. sold, originated, denied, withdrawn) including purchaser type or reasons for denial, and information on regional demographics. Moreover, the dataset allows to distinguish beween supply and demand effects in the mortgage loan market. The information whether the loan has been sold in the calender year of origination is very valuable in our definition of actual risk-taking. Since approximately 60% of originated mortgage loans are securitized (Loutskina and Strahan, 2009), we need to distinguish in our analyses between loans that have been sold and loans that have been held on balance sheet at least for a certain time period, because the former do not represent actual balance sheet risk-taking.[20] A major disadvantage of the HMDA dataset is that it does not provide more precise information on the time of loan application, purchase, or origination than the calendar year.

We obtain all loan applications for the years 2009 to 2011 from the FFIEC.[21] We remove three sub-samples from the raw data: First, we exclude all loan applications that have been denied in the pre-approval process, withdrawn or not accepted by the loan applicant or closed for incompleteness to focus on those loans that have either been approved and originated or denied in the loan approval process. Second, we drop all purchased loans from the sample to focus on true loan origination (and to avoid double-counting of loans as the dataset does not allow for exact matching of sold and purchased loans). Finally, we eliminate all loan applications with the purpose to refinance an existing loan because these loans usually have a different pricing and underwriting structure than new home purchase or home improvement loans (Avery et al., 2007).[22] We supplement the HMDA dataset with data on the regional housing price index obtained from the Federal Housing Finance Agency. We match the annual

[20] However, loans that remain on balance sheet do not necessarily represent balance sheet credit risk either, as lenders can issue synthetic collateralized debt obligations on their loan portfolio to insulate credit risk while still retaining loan servicing. The HMDA dataset does not provide information on synthetic collateralized debt obligations. As a robustness check we calculate the ratio of mortgage loans securitized but with servicing retained to total mortage loan portfolio from the bank level data and exclude all banks where this ratio is larger than 30%.

[21] This period is marked by a fall in house prices following the subprime mortgage crisis. We pay heed to account for these adverse conditions as well as varying developments of the regional housing markets by adding regional housing market controls and regional fixed effects.

[22] Moreover, refinancing loans could be biased due to 'evergreening' effects: Refinancing loans can exhibit a higher risk pattern overall when they are intended to prolong non-performing home purchase loans that would be otherwise written off.

appreciation as well as the avarage annual level of the housing price index based on the Metropolitan Statistical Area (MSA) in which the property is located.[23] In a final step, we match this dataset with the bank level dataset based on an individual and universal bank identifier to identify treatment and control group and to derive bank control variables.[24] We use the bank level dataset since mortgage loans are almost exclusively made through bank subsidiaries or individual banks.[25] Panel C of Table 1 provides summary statistics for the resulting loan application sample.

Dependent variables We calculate the loan-to-income ratio (LIR) of each loan application as the main risk measure in the loan level dataset. The LIR represents the loan applicant's ability to repay the loan amount considering his gross annual income and indicates riskier loans by increasing loan-to-income ratios. This measure is commonly used in the mortgage business to assess borrower risk, e.g. it is a criterion for eligibility for loans insured by the Federal Housing Administration. According to Dell'Ariccia et al. (2012), the measure is also used in lenders' loan decision processes. It usually correlates strongly with other measures of individual loan risk: As shown by Rosen (2011), loans with lower loan-to-income ratio tend to have better FICO scores.[26] Therefore, we are confident that the loan-to-income ratio is an appropriate risk measure in our loan sample. Since the distribution of the loan-to-income ratio displays some distant outliers on the high end, we drop all loan observations with loan-to-income ratio above the 99.5th percentile to avoid that our results are driven by those outliers.[27] We do this trimming for the sample of loan applications as well as for the sample of originated loans, so that the loan-to-income ratio ranges between 0 and 7.2 in our prepared sample. For the sample with originated loans, we use the loan-to-income ratio as the dependent variable. For the sample of loan applications, we exploit an approach similar to Duchin and Sosyura (2012). We simulate risk ranges by dividing the full loan application sample into ranges with $\Delta = 0.5\,LIR$ (0.0-0.5 being the safest and $>$3.0 the riskiest loan-to-income range) and run our multivariate baseline model regression for each range separately with the loan approval indicator as dependent variable. The loan approval indicator is set to 1, if a loan application has been approved and originated and 0, if the loan application has been denied. To rule out that our results are driven by loan demand than rather than by loan supply, we calculate the natural log of the total number of loan applications received by a bank from each loan-to-income range in each year and run our multivariate baseline model regression with this dependent variable as in Duchin and Sosyura (2012).

Explanatory variables and controls We use the same explanatory variables in the loan level dataset as described above. To identify treatment and control group in the loan level dataset, we use the treatment dummy $AFFECTED_i$ with the previously mentioned 10%/30% non-FDIA-regulated asset share cutoffs. We also utilize the treatment dummy with different cutoffs as a robustness check and construct a continuous variable exploiting the share of non-FDIA-regulated assets. To distinguish before and after treatment periods, we set the variable $afterOLA$ to 1 for all loan applications in 2011 and to 0 for all loan applications in 2009.[28]

We control for several groups of additional covariates that might influence risk-taking in the new mortgage loan business. First, we also use the set of bank control variables described above to ac-

[23] We use data for State Nonmetropolitan Areas when information on MSA is missing.

[24] HMDA does not provide these indentifiers for loans in 2009. We use identifiers from 2010 and 2011 and match lenders manually based on name and address when lenders are only present in the 2009 sub-sample.

[25] We identify two lenders with BHC-status. For consistency reasons we exclude those observsations from our analyses.

[26] FICO scores are provided by the Fair Isaac Corporation and measure a borrower's creditworthiness before obtaining a mortgage loan.

[27] We assume that these outliers mostly stem from misentries due to observed unrealistically high requested loan amounts or very low annual incomes.

[28] Since the calendar year is the only time designation in the HMDA dataset, we cannot match loans to particular quarters. Due to current data availability from the FFIEC, we could not obtain loan applications for years prior to 2009.

count for bank size, capital adequacy, profitability, and liquidity. To capture further individual bank characteristics, we exploit bank fixed effects. Second, we add dummy variables to control for certain loan characteristics that indicate whether the loan has been sold and whether the loan is government-guaranteed or -insured.[29] Third, we control for demographic conditions by adding the log of total population and the share of minority population for each U.S. Census Tract. Fourth, we take into account economic conditions - especially the state of the housing markets - as these can significantly vary across U.S. regions. We control for the log of median family income as well as change and average level of the house price index for each MSA. To capture further heterogeneity in demographic and economic conditions that is not time-varying, we use regional fixed effects on a very detailed geographical level, namely U.S. Census Tract (tract).

[29]Certain borrowers can receive loans that are insured by the Federal Housing Administration or guaranteed by the Veterans Administration, Farm Service Agency, or Rural Housing Services. Historically, these programs have allowed lower income U.S. borrowers to obtain mortgage loans that they could otherwise not afford.

Table 1: **Summary statistics**

Panel A: BHC sample

Variable group and name	Source	Mean	SD	Min	Max	N
Dependent variables (risk and business model)						
Bank z-score	BHC	4.57	(1.27)	-2.76	11.96	46043
σ RoA	BHC	19.09	(54.99)	0	2709	77613
Asset risk (RWA/assets)	BHC	73.08	(11.98)	0	126.2	15395
Trading assets ratio	BHC	0.33	(2.29)	0	42.75	14663
Low risk securities ratio	BHC	0.21	(2.91)	0	100	15547
High risk securities ratio	BHC	2.46	(9.37)	0	97.81	8797
CRECD loans ratio	BHC	0.48	(1.64)	0	31.32	15642
Deposit funding ratio	BHC	67.66	(13.41)	0	99.81	14663
Non-interest income ratio	BHC	23.56	(14.29)	0.03	99.53	16679
Explanatory variables						
BHC non-FDIA-regulated share	BHC, SDI	12.23	(9)	0	100	46569
Affected bank dummy (treatment)	BHC, SDI	0.05	(0.22)	0	1	19467
After OLA dummy		0.49	(0.5)	0	1	86038
Additional bank- and quarter-varying control variables						
Total assets (in USD mn)	BHC	5040.52	(72044.57)	0	2358266	49112
Capital ratio	BHC	10.04	(6.55)	-57	100	47410
Earnings (RoA)	BHC	0.1	(0.84)	-41.95	81.82	47359
Liqudity ratio	BHC	6.57	(6.61)	0.02	97.12	44375
CPP recipient bank-quarter	TR	0.03	(0.18)	0	1	86038

Panel B: Bank sample

Variable group and name	Source	Mean	SD	Min	Max	N
Dependent variables (risk and business model)						
Bank z-score	SDI	4.44	(1.17)	-9.46	8.83	126104
σ RoA	SDI	25.58	(50.23)	0	2014.1	126427
Asset risk (RWA/assets)	SDI	67.67	(14.72)	0	231.97	127022
Trading assets ratio	SDI	0.07	(1.11)	0	77.17	126936
Low risk securities ratio	SDI	71.36	(26.25)	0	100	123346
High risk securities ratio	SDI	1.86	(9.17)	0	100	112917
CRECD loans ratio	SDI	32.89	(20.88)	0	112.5	126209
Deposit funding ratio	SDI	69.29	(11.45)	0	98.66	126785
Non-interest income ratio	SDI	16.41	(12.65)	0	99.95	122973
Explanatory variables						
BHC non-FDIA-regulated share	BHC, SDI	7.68	(9.18)	0	100	89547
Affected BHC dummy (treatment)	BHC, SDI	0.03	(0.16)	0	1	56464
After OLA dummy		0.47	(0.5)	0	1	127170
Additional bank- and quarter-varying control variables						
Total assets (in USD mn)	SDI	1703319.62	(31321571.09)	66	1842568960	127170
Capital ratio	SDI	11.72	(7.37)	-13.52	100	126788
Earnings (RoA)	SDI	0.11	(1.02)	-28.38	93.5	126788
Liqudity ratio	SDI	7.31	(7.93)	0	100	126936
CPP recipient bank-quarter	TR	0.03	(0.17)	0	1	127170

Continued on next page

Panel C: Loan application sample

Variable group and name	Source	Mean	SD	Min	Max	N
Dependent variables						
Loan-Income-Ratio (loan appl.)	HMDA	2.04	(1.37)	0	7.22	4145701
Loan-Income-Ratio (orig. loans)	HMDA	2.15	(1.29)	0	7.22	3106212
Loan-Income-Ratio (sold loans)	HMDA	2.5	(1.13)	0.01	7.22	2021819
Loan-Income-Ratio (unsold loans)	HMDA	1.5	(1.31)	0	7.22	1084393
Approval indicator	HMDA	0.75	(0.43)	0	1	4329647
Explanatory variables						
BHC non-regulated share (continuous)	BHC, SDI	0.23	(0.21)	0	1	4089198
BHC non-regulated share (dummy)	BHC, SDI	0.42	(0.49)	0	1	1876201
After OLA (2011/2009)		0.46	(0.5)	0	1	4329647
Additional bank control variables						
Total assets (in USD mn)	SDI	401968.92	(564608.08)	18.13	1788146.13	4329291
Capital ratio	SDI	10.19	(2.6)	-1.01	40.2	4329224
Earnings (RoA)	SDI	0.12	(0.32)	-6.08	2.36	4329224
Liqudity ratio	SDI	5.69	(3.93)	0	77.74	4328745
CPP recipient bank	TR	0.57	(0.49)	0	1	4329647
Additional loan, demographic and economic control variables						
Government-guaranteed/-insured loan	HMDA	0.3	(0.46)	0	1	4329647
Sold loan (orig. loans)	HMDA	0.63	(0.48)	0	1	3242987
Total population in tract	HMDA	5487.1	(2676.24)	1	36146	4280501
Minority population in tract	HMDA	23.97	(25.29)	0.23	100	4280395
Median family income (in USD)	HMDA	65698.53	(14446.18)	16100	111900	4280666
House price index level in MSA	FHFA	183.56	(28.94)	110	338.02	4228877
House price index appreciation in MSA	FHFA	-3.67	(3.72)	-19.49	9.21	4228877

Notes: This table reports variable names, sources, means, standard deviations, minimum and maximum values, and the number of observations for which data is available in our sample. The sources are: FED Chicago BHC database (BHC), Federal Housing Finance Agency (FHFA), Home Mortgage Disclosure Act Loan Application Registry (HMDA), FDIC SDI database and call reports (SDI), U.S. Department of the Treasury (TR).

5 Results and robustness

This section presents and discusses our main results. We start from the effect of the improvement in resolution technology on overall bank risk, and continue elaborating effects on bank business model and loan decisions. These results are complemented by several extensions, e.g. testing the parallel trend assumption using a placebo treatment event, tests for too-big-to-fail effects, as well as a search for gambling behavior. Finally, we also discuss a set of robustness checks.

5.1 Overall bank risk-taking

In a very first step, we test the hypothesized effect of the OLA as an improvement in resolution technology on overall bank risk, using a univariate version of our baseline model. Table 2 presents the results of these univariate difference-in-difference comparisons, with Panel A focusing on a sample containing individual bank data and Panel B comprising a sample of aggregated BHC data. The treatment group includes all institutions that are particularly affected by the OLA, and is defined as all banks (or BHCs in Panel B) belonging to a BHC with more than 30% of its assets not subject to the FDIA resolution procedure. Conversely, the control group contains non-affected institutions, i.e. all independent banks (that are hence fully subject to the FDIA resolution regime) and banks (or BHCs) that are part of a holding with 10% or less non-FDIA-regulated assets.

For both the affected and non-affected institutions, we compute the means of the overall bank risk measures before (Q3 2007 - Q2 2009) and after (Q3 2010 - Q2 2012) the introduction of the Orderly Liquidation Authority. The resulting differences are tested for their statistical significance and displayed in columns (3) and (6). As a first result, it is interesting to note that all measures of overall bank risk are significantly decreasing across the board - for treatment and control groups on both bank and BHC level - between the pre- and the post-treatment periods. This, however, is not necessarily driven by the changes in regulation. Rather, it could be an overall trend towards less risk-taking that is influenced by, e.g. macroeconomic trends.[30] In order to test our hypothesis of a significant difference between treatment and control groups, we compute the univariate difference-in-difference results in column (7). Interestingly, for both the z-score and σRoA measures, the treatment group experiences a significantly larger decline in overall risk between pre- and post-treatment as compared to the control group. This finding is fully in line with our main hypothesis. However, the picture for the asset risk measure is less conclusive, as we do not find a significant effect in the univariate difference-in-difference estimates. Hence, these results may at most be interpreted as suggestive evidence - and we need to proceed with more conclusive tests.

Since these results may also be driven by unobserved variables, we run multivariate difference-in-difference estimations, adding two sets of fixed effects capturing both individual bank effects and quarter effects as well as a set of time-variant control variables as outlined in the previous section.[31]

Panel A of Table 3 presents the results of these multivariate difference-in-difference estimations.[32] These results show a highly significant decline in overall risk between pre- and post-treatment for affected banks as compared to non-affected banks. In particular, the coefficient on the interaction term $afterOLA_t * AFFECTED_i$ is positive for the z-score (i.e. more stable), negative for σRoA and asset risk (i.e. less volatile/risky), and statistically significant at the 1 percent level for all risk measures. These results hold both at the level of individual banks as well as on the level of BHCs and strongly support our main hypothesis. Beyond statistical significance, the results also suggest an economically considerable impact: Affected banks increase their z-score, for example, by more than 15%, which is about 3 times as much as non-affected banks.

In order to move beyond the arbitrary cutoffs defining the treatment and control groups, we also estimate our model by replacing the treatment dummy with the actual share of assets not subject to FDIA resolution. As before, we included bank and time fixed effects as well as time-variant controls in our estimation. The results are displayed in Panel B of Table 3 and are very much in line with our dummy results in Panel A. Again, the coefficient on the interaction term indicates a significant increase in overall bank stability and a significant decrease in overall bank risk. We also estimated alternative cutoffs (e.g. 50 vs. 10 percent non-FDIA-regulated share of business) as robustness tests, which are not reported but consistent with our main hypothesis.

The analyses presented so far have shown a significant difference-in-difference effect, indicating that risk-taking decreases with the degree to which a bank is affected by the improvement of resolution technologies. However, the validity of the difference-in-difference approach also relies upon the identifying

[30] One could, for example, argue that the outbreak of the financial crisis in 2008 increased volatility and that markets calmed down after 2010, which causes the effect we find.

[31] Note that for brevity of the tables, we do not report the regression coefficients on all of these control variables (which are generally in line with expectations and previous empirical findings).

[32] Note that the level effect on the $afterOLA_t$ dummy drops as it is captured by the time fixed effects.

Table 2: **Bank risk-taking: Univariate Difference-in-Difference analyses**

Panel A: Bank level

	(1)	(2)	(3)=(2)-(1)	(4)	(5)	(6)=(5)-(4)	(7)=(3)-(6)
	Affected banks			**Non-affected banks**			
	Before OLA	**After OLA**	**Dif**	**Before OLA**	**After OLA**	**Dif**	**Dif-in-Dif**
Dep. variable							
Z-score	4.086	4.741	0.655***	4.270	4.440	0.170***	0.485***
			(0.0608)			(0.0108)	(0.0668)
σ **RoA**	0.521	0.234	-0.287***	0.321	0.252	-0.0697***	-0.218***
			(0.0349)			(0.00503)	(0.0312)
Asset risk	0.694	0.631	-0.0618***	0.681	0.630	-0.0517***	-0.0101
			(0.0014)			(0.00132)	(0.00822)

Panel B: BHC-level

	(1)	(2)	(3)=(2)-(1)	(4)	(5)	(6)=(5)-(4)	(7)=(3)-(6)
	Affected banks			**Non-affected banks**			
	Before OLA	**After OLA**	**Dif**	**Before OLA**	**After OLA**	**Dif**	**Dif-in-Dif**
Dep. variable							
Z-score	4.051	4.554	0.503***	4.17	4.37	0.196***	0.307***
			(0.0896)			(0.0202)	(0.0986)
σ **RoA**	1.119	0.409	-0.71***	0.214	0.193	-0.0212***	-0.689***
			(0.196)			(0.00477)	(0.0475)
Asset risk	0.697	0.632	-0.0644***	0.762	0.682	-0.0801***	0.0157
			(0.0159)			(0.00292)	(0.0109)

Notes: This table presents univariate difference-in-difference estimates. Panel A reports the results for the bank sample, Panel B for the bank holding company (BHC) sample. Banks (or BHCs) are classified into two groups. The treatment group comprises affected banks (BHCs) that are part of a BHC with more than 30% of non-FDIA-regulated assets. The control group comprises non-affected banks (BHCs) that are independent or part of a BHC with less than 10% of non-FDIA-regulated assets. Treatment is defined as the introduction of the Orderly Liquidation Authority (OLA). Several measures of overall bank risk are taken as dependent variables: *z-score* (defined as mean return on assets plus capital ratio divided by the standard deviation of return on assets), σ *RoA* (defined as standard deviation of return on assets), and *asset risk* (defined as risk-weighted assets divided by total assets). Difference-in-difference estimates are displayed in column (7).
Heteroskedasticity consistent standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: **Bank risk-taking: Multivariate Difference-in-Difference analyses**

Panel A: Dummy variable (treatment and control group definition)

	(1)	(2)	(3)	(4)	(5)	(6)
Level	**Bank level**			**BHC-level**		
Dep. variable	**Z-score**	**σ RoA**	**Asset risk**	**Z-score**	**σ RoA**	**Asset risk**
Affected bank	0.131**	0.0459	0.0142			
	(0.0559)	(0.0285)	(0.00903)			
Affected BHC				-0.991***	-0.0649	-0.195
				(0.253)	(0.148)	(0.141)
Affected bank x after OLA	0.476***	-0.181***	-0.0220***			
	(0.0410)	(0.0277)	(0.00536)			
Affected BHC x after OLA				0.545***	-0.504***	-0.0131**
				(0.0730)	(0.153)	(0.00645)
Constant	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	55,811	55,894	56,140	17,726	17,995	5,560
R-squared	0.813	0.810	0.889	0.858	0.717	0.894

Panel B: Continuous variable (unregulated share in %)

	(1)	(2)	(3)	(4)	(5)	(6)
Level	**Bank level**			**BHC-level**		
Dep. variable	**Z-score**	**σ RoA**	**Asset risk**	**Z-score**	**σ RoA**	**Asset risk**
Unregulated share (parent BHC-level)	0.390***	0.0151	0.0675***			
	(0.0673)	(0.0277)	(0.00948)			
Unregulated share (BHC-level)				-0.869***	0.162	-0.110
				(0.244)	(0.202)	(0.0775)
Unregulated share x after OLA	0.772***	-0.133***	-0.0635***			
	(0.0537)	(0.0276)	(0.00690)			
Unregulated share x after OLA				1.766***	-1.316***	-0.0338*
				(0.155)	(0.391)	(0.0199)
Constant	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	88,710	88,795	89,194	43,050	43,338	14,221
R-squared	0.786	0.797	0.885	0.809	0.743	0.877

Notes: This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on overall bank risk. Panel A reports the results for the difference-in-difference estimation, Panel B for the estimation using a continous explanatory variable interaction. *Affected bank (BHC)* takes a value of 1 if the bank (BHC) is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank (BHC) is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *Unregulated share* is defined as the share of non-FDIA-regulated assets. *After OLA* is 1 for the quarters Q3 2010 - Q2 2012 and 0 for the quarters Q3 2007 - Q2 2009. Several measures of overall bank risk are taken as dependent variables: *z-score* (defined as mean return on assets plus capital ratio divided by the standard deviation of return on assets), σ *RoA* (defined as standard deviation of return on assets), and *asset risk* (defined as risk-weighted assets divided by total assets). Control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, liquidity ratio, and an indicator variable that takes the value of 1 if the bank was a recipient of the TARP CPP program in a respective quarter (and 0 otherwise). All models include bank and time fixed effects.

Heteroskedasticity consistent standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

assumption of a parallel trend between treatment and control group in the absence of treatment. While we presented some suggestive evidence underlining this assumption in the previous section, we now apply a more rigorous approach in testing it. In order to do so, we extend our dataset to cover another 8-quarter period stretching from Q3 2005 to Q2 2007, which we define as a pre-placebo period. We now test the effect of a placebo treatment between the pre-placebo period and the pre-treatment period, using essentially the same model as in the analyses above. If the parallel trend assumption holds, we expect not to find a significant difference-in-difference effect between the affected and non-affected banks or BHCs across both periods. The results of this placebo test are displayed in Table 4. Indeed, there is no significant difference-in-difference effect to be found for the z-score and asset risk measures, neither in the bank nor in the BHC panel. While the coefficient on the interaction is also insignificant with σRoA as dependent variable in the BHC sample, return volatility seems to increase for individual banks belonging to an affected BHC after the placebo treatment. A potential explanation why it is the return volatility (and just the return volatility) that is significantly higher for affected banks could be offered by the rational behavior that we would presume for these banks: As there is a lower threat of resolution for these banks before the enactment of the OLA, they had incentives to take on higher risks during the pre-placebo period (and before). When the financial crisis hits (which coincides with the placebo treatment), this additional risk materializes in an overproportional increase of volatility. Admittedly, this is only a vague explanation and further research is warranted to investigate this effect. Apart from this one reaction of σRoA, however, the presented evidence is mostly consistent with the parallel trend assumption.

Furthermore, it is interesting to note that the level effects for affected BHCs seem to confirm the presumption of higher overall risk of this group previous to the introduction of the OLA. This is consistent with our hypothesis that holdings with high unregulated shares are less subject to FDIA resolution and hence enjoy more of an implicit bailout guarantee - previous to OLA. The effect does not occur for individual banks, presumably as these were already subject to FDIA resolution, even if they were part of a BHC (implying that BHC risk-taking was largely done through the non-FDIA-regulated parts). When the resolution threat becomes realistic for banks and BHCs alike (even if they hold high previously non-FDIA-regulated shares), the difference in risk-taking and business model decisions is hypothesized to occur both in affected banks and affected BHCs - which is remarkably consistent with the results in previous and following tables.

Taken together, the results presented so far confirm our main hypothesis: Banks or BHCs that were largely not subject to the FDIA resolution regime before are particularly affected by the introduction of the OLA and decrease their overall risk accordingly. We now want to go beyond the measures of overall bank risk and analyze in more detail how banks change their behavior with regard to business model and investment choices as well as new loan origination.

5.2 Bank business model choices and loan origination

As outlined above, we define and compute several indicators for bank business model and investment choices, that have been suggested in the literature (Brunnermeier et al., 2012; DeJonghe, 2010; DeYoung, 2013; Duchin and Sosyura, 2012). We test the difference-in-difference effect by using these indicators as dependent variables in our multivariate baseline model, including fixed effects and additional controls. Since data for these measures is in large parts only available at the bank level (particularly for the loan data), we carry out all of our tests for the bank dataset. Table 5 presents the results, which are all consistent with the hypothesized decrease in risky activities and investment choices for the affected banks after the introduction of the OLA. We start with the effect on the trading assets ratio (column (1)). In line with the expectation that affected banks decrease risky and volatile activities (such as proprietary trading), we find a negative and significant coefficient on the interaction term. A

Table 4: **Bank risk-taking: Multivariate Difference-in-Difference analyses with placebo test**

	(1)	(2)	(3)	(4)	(5)	(6)
Level	**Bank level**			**BHC-level**		
Dep. variable	**Z-score**	**σ RoA**	**Asset risk**	**Z-score**	**σ RoA**	**Asset risk**
Affected bank	0.160**	-0.0706	-0.00704			
	(0.0639)	(0.0468)	(0.00900)			
Affected BHC				-1.084***	0.382**	0.0586**
				(0.242)	(0.169)	(0.0237)
Affected bank x after placebo	-0.0177	0.106***	0.00590			
	(0.0367)	(0.0214)	(0.00362)			
Affected BHC x after placebo				0.0699	0.172	0.000800
				(0.0804)	(0.131)	(0.00473)
Constant	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	62,757	62,792	63,122	20,017	20,075	7,740
R-squared	0.755	0.819	0.901	0.787	0.774	0.933

Notes: This table presents multivariate difference-in-difference estimates for a placebo treatment. *Affected bank (BHC)* takes a value of 1 if the bank (BHC) is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank (BHC) is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *After placebo* is 1 for the quarters Q3 2007 - Q2 2009 and 0 for the quarters Q3 2005 - Q2 2007. Several measures of overall bank risk are taken as dependent variables: *z-score* (defined as mean return on assets plus capital ratio divided by the standard deviation of return on assets), σ *RoA* (defined as standard deviation of return on assets), and *asset risk* (defined as risk-weighted assets divided by total assets). Control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, liquidity ratio, and an indicator variable that takes the value of 1 if the bank was a recipient of the TARP CPP program in a respective quarter (and 0 otherwise). All models include bank and time fixed effects.

Heteroskedasticity consistent standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5: **Bank business model and investment choices: Multivariate Difference-in-Difference analyses**

Level	(1) Bank level	(2)	(3)	(4)	(5)	(6)
Dep. variable	**Trading assets ratio**	**Low risk securities ratio**	**High risk securities ratio**	**CRECD loan ratio**	**Deposit funding ratio**	**NII ratio**
Affected bank	0.00101	-0.0171	0.0404***	-0.00354	-0.0109	-0.000635
	(0.00318)	(0.0225)	(0.0151)	(0.00862)	(0.00720)	(0.00647)
Affected bank x after OLA	-0.00605***	0.0584***	-0.0377***	-0.0108***	0.0307***	-0.00927**
	(0.00136)	(0.0118)	(0.00926)	(0.00312)	(0.00610)	(0.00447)
Constant	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	56,140	54,000	44,050	55,384	56,137	53,737
R-squared	0.776	0.778	0.784	0.961	0.907	0.921

Notes: This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on bank business model and investment decisions. *Affected bank* takes a value of 1 if the bank is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *After OLA* is 1 for the quarters Q3 2010 - Q2 2012 and 0 for the quarters Q3 2007 - Q2 2009. Several measures of bank business model and investment decisions are taken as dependent variables: *trading asset ratio* (defined as ratio of assets held in trading accounts to total assets), *low risk securities ratio* (defined as the ratio of securities of U.S. government agencies and subdivisions to total investment securities), *high risk securities ratio* (defined as the ratio of equity securities, asset-backed securities, and trading accounts to total investment securities), *CRECD loan ratio* (defined as the sum of commercial real estate loans and construction and development loans, divided by total loans), *deposit funding ratio* (defined as deposits divided by assets), and *non-interest income ratio* (defined as average interest income divided by average total income). Control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, liquidity ratio, and an indicator variable that takes the value of 1 if the bank was a recipient of the TARP CPP program in a respective quarter (and 0 otherwise). All models include bank and time fixed effects.
Heteroskedasticity consistent standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

similar result holds for the effect on the low and high risk securities ratios, presented in columns (2) and (3): While affected banks seem to decrease investments in risky securities, they appear to increase their exposure towards low risk securities classes. This shift in the securities portfolios is consistent with the expectation that affected banks will rush for safer investments and business models after the introduction of the OLA. In a similar vein, we would expect the treatment group of banks to decrease their exposure towards highly complex and risky loans (such as the CRECD loans) relatively to their total loan portfolio. The negative and significant coefficient on the difference-in-difference term in column (4) suggests that we cannot reject this hypothesis.

Turning to the liability side of the bank business model, we would expect affected banks to opt for sources of funding that are considered more stable and carry less interest rate risk. If the deposit funding ratio correctly proxies for this, we find our expectation confirmed by a positive and significant coefficient on the interaction term. Finally, we look at the effect on the sources of income of the bank. The negative coefficient on the interaction term in column (6) suggests that affected banks decrease their non-interest income relative to interest income stronger than the control group after the introduction of the OLA. If non-interest income is indeed more volatile and associated with overall (systemic) risk as claimed in the literature, the results found in column (6) are consistent with our main hypothesis.

The data and evidence presented so far largely draws upon aggregated accounting data. In order to complement this with actual risk-taking in business operations on banks' micro-level, we extend our analysis to the mortgage loan business. We use our multivariate baseline model to test the difference-in-difference effect on risk-taking in newly originated mortgage loans. Table 6 presents the results exploiting the loan-to-income ratio as the risk measure. Column 1 displays an analysis on the whole sample of newly originated loans, yielding a negative and significant coefficient on the interaction term that confirms our main hypothesis. In a second step, we split this sample into loans that have been sold in the same calendar year (column (2)) and loans that have not been sold in the same calendar year (column (3)). We assume that loans in the latter sample have been held on balance sheet at least for a certain time period, so that they measure risk-taking more accurately. We find that affected banks significantly decrease loan-to-income ratios of new loans after the introduction of OLA for both sold and unsold loans.

One further caveat could be loans that remain on the balance sheet for servicing but are de facto securitizied (e.g. through synthetic collateralized debt obligations) and hence do not necessarily represent risk-taking. Since the HMDA dataset does not provide information on synthetic collateralized debt obligations, we calculate the ratio of mortgage loans securitized but with servicing retained to total mortgage loan portfolio from the bank level dataset and exclude all banks where this ratio of synthetic loans is larger than 30%. We rerun our multivariate baseline model and find that affected banks with a low share of synthetic loans in fact reduce the risk of new loans that remain on their balance sheet after the introduction of OLA, while this effect is not significant for sold loans (see Panel B of Table 6).

It could be possible that our results on the sample of originated loans stem from loan demand rather than loan supply effects, i.e. only high quality borrowers demand loans from affected banks after the introduction of OLA. To account for potential loan demand effects, we include rejected loan applications, split the loan application sample into different risk ranges based on the loan-to-income ratio, and test our main hypothesis using the application approval indicator as dependent variable. The results for the analysis on the approval rate of loan applications are shown in Panel A of Table 7. We find that the probability of loan approval by affected banks decreases after the introduction of OLA. However, this decrease is not significant for the safest risk range with loan-to-income ratio below 0.5, while it is significant for all remaining risk ranges. Interestingly, we find that in the pre-OLA period the approval rate is higher for affected banks than for non-affected banks. For non-affected banks, the approval rate also declines in the period after introduction of OLA, however only significantly in the safest loan-to-income ranges. Additionally, we test for systematic differences in loan demand across risk ranges by employing the total number of loan applications per bank, year, and risk range as dependent variable and find that the loan demand at affected banks did not significantly decrease after introduction of OLA (see Panel B of Table 7).

We bring forward evidence that after the introduction of the resolution threat, affected banks decrease risk-taking in new loan business by approving less loans from higher risk ranges and can exclude that our results are driven by loan demand effects. In sum, the presented results are consistent with the interpretation that affected banks decrease their overall risk-taking after the introduction of the Orderly Liquidation Authority and do so by shifting their investments, business models, and loan decisions towards more prudent behavior.

Table 6: **Risk taking in new mortgage loan business: Multivariate Difference-in-Difference analyses**

Panel A: Newly originated loans from all banks in sample

	(1)	(2)	(3)
Level	**Loan level**		
Sample	**All originated loans**	**Sold loans**	**Unsold loans**
Dep. variable		**Loan-to-income ratio**	
Affected bank	-0.685***	-0.170	-0.701***
	(0.0767)	(0.135)	(0.0984)
After OLA	0.00146	-0.0581***	0.0458***
	(0.00367)	(0.00506)	(0.00554)
Affected bank x after OLA	-0.0691***	-0.0352***	-0.0459***
	(0.00477)	(0.00603)	(0.00918)
Constant	YES	YES	YES
Bank controls	YES	YES	YES
Loan controls	YES	YES	YES
Demogr. controls	YES	YES	YES
Economic controls	YES	YES	YES
Bank FE	YES	YES	YES
Tract FE	YES	YES	YES
Observations	1,366,242	913,178	453,064
R-squared	0.324	0.219	0.367

Panel B: Newly originated loans from banks with share of synthetic loans <30%

	(1)	(2)	(3)
Level	**Loan level**		
Sample	**All originated loans**	**Sold loans**	**Unsold loans**
Dep. variable		**Loan-to-income ratio**	
Affected bank	-0.698***	-0.194	-0.747***
	(0.0824)	(0.136)	(0.110)
After OLA	-0.0193***	-0.0624***	0.00752
	(0.00514)	(0.00732)	(0.00769)
Affected bank x after OLA	-0.0470***	-0.0192	-0.0406***
	(0.00817)	(0.0118)	(0.0128)
Constant	YES	YES	YES
Bank controls	YES	YES	YES
Loan controls	YES	YES	YES
Demogr. controls	YES	YES	YES
Economic controls	YES	YES	YES
Bank FE	YES	YES	YES
Tract FE	YES	YES	YES
Observations	830,560	532,525	298,035
R-squared	0.350	0.229	0.387

Notes: This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on risk-taking in new originated mortgage loans. Panel A reports the results for the sample with all banks, Panel B restricts the sample to banks where the ratio of mortgage loans securitized but with servicing retained to total mortgage loan portfolio is less than 30%. Sold loans are originated loans that were sold in calendar year of origination; unsold loans are originated loans that were not sold in calendar year of origination. *Affected bank* takes a value of 1 if the bank is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *After OLA* is 1 for all loans originated in 2011 and 0 for all loans originated in 2009. The dependent variable to measure risk-taking in new loans is the *loan-to-income ratio*. Bank control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, and liquidity ratio. Loan control variables comprise two indicator variables: sold loan is equal to 1 if the loan has been sold (all originated loans sample) and guaranteed/insured loan is equal to 1 if the loan is guaranteed or insured by the government. Demographic control variables comprise the natural logarithm of total population in tract and share of minority population in tract. Economic controls comprise the natural logarithm of median family income in tract, appreciation and level of regional house price index. All models include bank and regional (tract) fixed effects.

Heteroskedasticity consistent standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 7: **Approval of mortgage loan applications and loan demand along risk ranges: Multivariate Difference-in-Difference analyses**

Panel A: Approval rate of loan applications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Level	**Loan level**							
		Loan applications within loan-to-income ratio range						
Sample	**All appl.**	**0.0-0.5**	**0.5-1.0**	**1.0-1.5**	**1.5-2.0**	**2.0-2.5**	**2.5-3.0**	**>3.0**
Dep. variable	**Application approval indicator**							
Affected bank	0.102***	0.0270	0.0901	0.157**	-0.0146	0.147*	0.172**	0.155*
	(0.0221)	(0.0532)	(0.0647)	(0.0640)	(0.0614)	(0.0872)	(0.0819)	(0.0936)
After OLA	-0.0043***	-0.0233***	-0.0117***	-0.00251	0.00423	-0.00275	-0.00112	0.00294
	(0.00103)	(0.00345)	(0.00358)	(0.00317)	(0.00266)	(0.00254)	(0.00272)	(0.00218)
Affected bank x after OLA	-0.0465***	-0.00640	-0.0167***	-0.0529***	-0.0630***	-0.0599***	-0.0540***	-0.0563***
	(0.00127)	(0.00491)	(0.00481)	(0.00406)	(0.00336)	(0.00319)	(0.00339)	(0.00253)
Constant	YES	YES	YES	YES	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES	YES	YES	YES	YES
Loan controls	YES	YES	YES	YES	YES	YES	YES	YES
Demogr. controls	YES	YES	YES	YES	YES	YES	YES	YES
Econ. controls	YES	YES	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	1,839,672	193,601	164,310	189,605	242,163	257,310	234,283	491,291
R-squared	0.443	0.425	0.446	0.469	0.493	0.514	0.539	0.581

Panel B: Total number of loan applications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Level	**Loan level**							
		Loan applications within loan-to-income ratio range						
Sample	**All appl.**	**0.0-0.5**	**0.5-1.0**	**1.0-1.5**	**1.5-2.0**	**2.0-2.5**	**2.5-3.0**	**>3.0**
Dep. variable	**Log of total number of loan applications per bank, year, and range**							
Affected bank	-0.196	0.605	-0.216	-0.420*	-0.230	0.101	-0.825***	-0.814**
	(0.180)	(0.410)	(0.278)	(0.242)	(0.299)	(0.313)	(0.242)	(0.341)
After OLA	-0.171***	-0.222***	-0.166***	-0.119***	-0.214***	-0.188***	-0.237***	-0.305***
	(0.0153)	(0.0269)	(0.0238)	(0.0247)	(0.0256)	(0.0253)	(0.0272)	(0.0297)
Affected bank x after OLA	-0.127	-0.229	-0.211	-0.198	-0.119	-0.109	-0.185	-0.0855
	(0.122)	(0.166)	(0.133)	(0.149)	(0.178)	(0.214)	(0.238)	(0.202)
Constant	YES	YES	YES	YES	YES	YES	YES	YES
Bank controls	YES	YES	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	33,762	4,510	4,492	4,338	4,225	4,060	3,791	4,261
R-squared	0.015	0.085	0.078	0.072	0.097	0.104	0.108	0.157

Notes: This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on approval rate of mortgage loan applications and loan demand along risk ranges. Column (1) shows the full sample of loan applications, columns (2)-(8) contain the sub-samples of loan applications based on loan-to-income ratio ranges. The dependent variable in Panel A is the *application approval indicator* which equals 1 when loan application succeeded in loan origination (and 0 when the application was denied). Panel B employs the natural logarithm of *total number of loan applications* per bank, year, and risk range as dependent variable. *Affected bank* takes a value of 1 if the bank is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *After OLA* is 1 for all loan applications in 2011 and 0 for all loan applications in 2009. Bank control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, and liquidity ratio. Loan control variables comprise two indicator variables: sold loan is equal to 1 if the loan has been sold and guaranteed/insured loan is equal to 1 if the loan is guaranteed or insured by the government. Demographic control variables comprise the natural logarithm of total population in tract and share of minority population in tract. Economic controls comprise the natural logarithm of median family income in tract, appreciation and level of regional house price index. Models in Panel A include bank and regional (tract) fixed effects; models in Panel B include bank fixed effects.

Heteroskedasticity consistent standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

5.3 Extensions and robustness

Is the OLA a credible threat for all banks or are there 'too-big-to-not-rescue' institutions? So far, we have tested our main hypothesis and found that the affected banks indeed reduced their risk-taking after the introduction of the OLA relative to non-affected banks. However, we also postulated in the beginning that this effect might vary with the credibility and effectiveness, and in particular with the political will to apply the new improvement in regulatory technology. Formulated in the context of the model by DeYoung et al. (2013): When the political will or preference for discipline is low or the liquidity trade-off is high, we expect to find a lower or even no effect of the introduction of the OLA on the behavior of affected banks. If financial institutions do not think that the OLA represents a credible threat, they will not change their behavior as a response to it.

One straightforward - and admittedly simple - way of testing this prediction is the 'too-big-to-not-rescue' effect. Essentially, we take systemic importance or sheer size of a bank as a proxy for high liquidity trade-off.[33] Winding down such an institution might produce high liquidity costs, making discipline less favored by regulators, which ultimately results in low credibility of the threat of resolution - even after the introduction of the OLA. Such institutions are 'too-big-to-not-rescue'. Hence, we test whether extraordinarily large institutions are less (or not at all) responsive to this improvement in resolution technologies. For robustness, we test two different definitions of systemic importance. For our first test, we isolate all banks that form part of one of the 8 U.S. financial holdings that have been determined 'global systemically important bank' (GSIFI) by the Financial Stability Board.[34]

As an alternative definition, we form a sample of all institutions with asset size larger than USD 50 billion. This is not an entirely arbitrary cutoff, but chosen according to a threshold above which the Dodd-Frank Act stipulates specific enhanced supervision activities and prudential standards, also in conjunction with the OLA (compare, e.g., DFA, Title II, Sec 210). We use these two definitions as they are alternative, yet not mutually repetitive indicators of systemic importance.[35] When we run our model on these separate samples of banks, we have to use the continuous version of the explanatory variable since too many institutions would be dropped from the sample otherwise. We are able to conduct these tests on our bank level sample, with the results being reported in Table 8.

In line with our expectations, the coefficients of the interaction term emerge to be insignificant for the return volatility as dependent variable in both subsamples. However, it is interesting to note that for the z-score and asset risk as dependent variable, the coefficients on the interaction term are significant, but in opposite direction as compared to our baseline regression results. We interpret this finding in a way that more affected systemically important banks do not reduce their risk-taking after the introduction of the OLA, but might even increase it. A possible explanation for this finding is that the threat of resolution resulting from the OLA is not credible for them. They do not seem to believe that the regulator is indeed fully enabled to resolve such institutions in case of failure - be it due to lacking financial or operational capabilities, fears for systemic risk and contagion, or other rationales. Moreover, as the OLA was considered the major change in bank resolution law in response to the financial crisis, it seems unlikely that these institutions had to expect a further, maybe more credible, upgrade in resolution technology any time soon. Imagining all financial institutions as a system of corresponding vessels in a situtation where most affected institutions have to reduce risk, there are only few players that can take this risk on - and these are the affected institutions for which the resolution

[33]For clarification: The 'affected' bank classification is so far not defined by size or interconnectedness, but purely on grounds of resolvability according to the FDIA. Hence, there are, e.g., large as well as small banks being classified as 'affected' (and 'not affected').

[34]In total, the Financial Stability Board designated 29 institutions to be GSIFI, 8 of which are of U.S. origin: Bank of America, Bank of New York Mellon, Citigroup, Goldman Sachs, JPMorgan Chase, Morgan Stanley, State Street, and Wells Fargo.

[35]Only 24 institutions in our Bank level sample fulfill both criteria, while additional 40 institutions form part of a GSIFI, and additional 80 institutions report more than USD 50 billion in assets.

Table 8: **Too-big-to-not-rescue effect: Multivariate Difference-in-Difference analyses on TBTNR banks**

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	**Part of U.S.-GSIFI**			**Asset size USD 50+ billion**		
Dep. variable	**Z-score**	**σ RoA**	**Asset risk**	**Z-score**	**σ RoA**	**Asset risk**
Unregulated share (parent BHC-level)	2.466***	-1.816*	0.721***	1.133***	-0.892***	0.111*
	(0.948)	(0.988)	(0.160)	(0.367)	(0.238)	(0.0579)
Unregulated share x after OLA	-1.415**	0.0800	0.262***	-0.815*	0.0992	0.0795*
	(0.696)	(0.295)	(0.0643)	(0.475)	(0.147)	(0.0455)
Constant	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	485	485	492	452	452	454
R-squared	0.824	0.665	0.925	0.863	0.847	0.907

Notes: This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on overall risk of those banks that could be classified as too-big-to-not-rescue. The estimation is conducted for two subsamples of banks: All banks that are part of one of the U.S. GSIFIs as classified by the FSB (columns (1) to (3)) and all banks with total asset size of USD 50 billion or more (columns (4) to (6)). *Affected bank* takes a value of 1 if the bank is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *After OLA* is 1 for the quarters Q3 2010 - Q2 2012 and 0 for the quarters Q3 2007 - Q2 2009. Several measures of overall bank risk are taken as dependent variables: *z-score* (defined as mean return on assets plus capital ratio divided by the standard deviation of return on assets), σ *RoA* (defined as standard deviation of return on assets), and *asset risk* (defined as risk-weighted assets divided by total assets). Control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, liquidity ratio, and an indicator variable that takes the value of 1 if the bank was a recipient of the TARP CPP program in a respective quarter (and 0 otherwise). All models include bank and time fixed effects.
Heteroskedasticity consistent standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

threat is still not credible. Hence, a rational strategy for these 'to-big-to-not-rescue'-institutions would be to not decrease, but increase risk-taking (at least as long as the resolution threat does not become more realistic). We cannot test this directly, but the shift in securities and trading asset holdings that we find in the aggregate is at least suggestive to this rationale: While most affected institutions that are not part of a GSIFI heavily reduce their securities holdings (particularly their high-risk securities and trading assets) after the introduction of the OLA, the affected GSIFI institutions even increase their holdings.

Gambling in the meantime? In a final extension, we would like to test how banks' risk-taking changed in the post-announcement period, i.e. between the proposal of the OLA (mid 2009) and its actual enactment (mid 2010). Theory and available empirical evidence suggest that gambling might occur in this period if the changes in regulation reduce affected banks' charter value (Fischer et al., 2012; Murdock et al., 2000). To the extent that the introduction of the OLA actually reduces the charter value of affected banks, e.g. by removing the previously existing implicit bailout guarantee, we might find evidence of gambling in bank behavior. However, banks would need to shift their behavior twice between the publicly known proposal of the OLA and its signing into law. First, after June 2009, they would need to increase risk to exploit the trade-off between high risk-return and potential failure with likely bailout on the one hand and loss in charter value on the other hand. Very briefly after this, before July 2010, bailout becomes less likely (as the OLA becomes effective), and banks would need to readjust their strategy again. Is this realistic? Was the time horizon long enough to shift the behavior twice or was the legislation passed so quickly that gambling did not occur?

In order to test for the occurrence of gambling in the intermediate phase, we define a post-treatment period ($afterOLA$) and a 'gambling-period' ($afterannouncement$) that we run against a pre-treatment period in turn. While the pre-treatment period is defined as Q3 2008 to Q2 2009, the period in which gambling might happen stretches from Q3 2009 to Q2 2010. For comparison, we define another 4-quarter post-treatment period as Q3 2010 to Q2 2011 that we use as a benchmark effect to compare it to the gambling results. For robustness, we define an additional set of pre-, gambling-, and post-treatment periods, which stretch over 2 quarters each: Q1/Q2 2009 as pre-treatment period, Q3/Q4 2009 as potential gambling period, and Q3/Q4 2010 as post-treatment period. We run the main model with the z-score[36] (for overall comparison) as well as a selection of investment choice risk measures that we deem to be adjustable within a short period, i.e. the trading asset ratio as well as the low and high risk securities ratios, as dependent variables.[37] Panel A in Table 9 presents the results for the 4-quarter and 2-quarter benchmark regressions (pre- vs post-treatment). It should be noted that these results can also be interpreted as a robustness test of the initial 8-quarter results. With all overall and investment risk measures indicating less risk-taking by affected banks after the introduction of the OLA in the 4-quarter/2-quarter regressions, these results are fully in line with our baseline model. The findings about potential gambling are displayed in Panel B of Table 9 (pre- vs. gambling-period). Interestingly, we do not find a significant effect in the overall risk regression using the z-score as dependent variable. Likewise, the coefficient on the interaction term is not significant for the trading assets ratio. However, the results for low and high risk securities ratios (for the 2-quarter periods only the high-risk securities) indicate that, if at all, affected banks take less - not more - risk in the intermediate period. Interpreting these findings, we do not find any evidence for gambling in the intermediate period - rather, affected banks even start decreasing their risk-taking already by shifting their securities portfolio.

How robust are these findings? In order to test the robustness of the results presented above, we have conducted a host of robustness tests, using alternative specifications and variable definitions, sample restrictions, and additional entire datasets. This section briefly summarizes the robustness tests and their main results. For brevity and ease of comparison, some of the results from the robustness tests were already presented in the respective tables above. All other results - although not presented - are also largely consistent with our hypotheses and confirm the effects we report.

The following robustness tests have been carried out:

- With regard to our dependent variables, we have defined and tested a set of alternative measures for overall bank risk and risk choices in business model/investment decisions, both on the bank level and on the micro-level of business decisions. All of our results have been shown to be robust to these alterations and yield similar conclusions, indicating that the results are not driven by specific definitions of individual dependent variables but largely consistent with each other.

- We acknowledge that the dummy-version of our treatment variable $AFFECTED_i$ is defined along arbitrary cutoffs. In order to test the robustness of our main bank risk-taking results, we also defined alternative cutoffs (0%, 5%, 10% on the lower bound and 30% and 50% on the upper bound).[38] Moreover, we also used the share of non-FDIA-regulated assets as explanatory variable,

[36] Note that we cannot reasonably define the z-score for the 2-period regressions as it requires the computation of a mean and a standard deviation (for which we defined a minimum requirement of 3 available datapoints above). Hence, z-score results are only presented for 4-quarter period regressions.

[37] We tested the two models with all other previously used dependent variables as well and find no immediate adjustment effect for the intermediate period.

[38] Concerning the loan level dataset, varying the lower cutoff bound yields similar results. Applying a 50% cutoff for the upper bound is not meaningful as there are only very few banks in the loan level dataset with share of non-FDIA-regulated assets above this cutoff.

Table 9: **Bank risk-taking and business model choices: Multivariate Difference-in-Difference analyses with 4-quarter periods and test of risk-taking in post-announcement period**

Panel A: Benchmark tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Level	**Bank level**						
Periods	**4-quarter periods**				**2-quarter periods**		
Dep. variable	**Z-score**	**Trading assets ratio**	**Low risk securities ratio**	**High risk securities ratio**	**Trading assets ratio**	**Low risk securities ratio**	**High risk securities ratio**
Affected bank	0.0889	0.00313***	-0.0240	0.0591**	0.00315	-0.0253	0.125**
	(0.128)	(0.00115)	(0.0403)	(0.0278)	(0.00273)	(0.0886)	(0.0514)
Affected bank x after OLA	0.252***	-0.00568***	0.0542***	-0.0482***	-0.00390*	0.0517***	-0.0515***
	(0.0600)	(0.00202)	(0.0145)	(0.0129)	(0.00202)	(0.0170)	(0.0148)
Constant	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES
Observations	28,393	28,579	27,513	21,860	14,597	14,045	11,221
R-squared	0.801	0.749	0.850	0.838	0.801	0.892	0.883

Panel B: Gambling tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Level	**Bank level**						
Periods	**4-quarter periods**				**2-quarter periods**		
Dep. variable	**Z-score**	**Trading assets ratio**	**Low risk securities ratio**	**High risk securities ratio**	**Trading assets ratio**	**Low risk securities ratio**	**High risk securities ratio**
Affected bank	0.0882	-0.000280	-0.0225	0.0131	-0.00269	-0.0493	0.0269**
	(0.133)	(0.00162)	(0.0271)	(0.0206)	(0.00430)	(0.0328)	(0.0119)
Affected bank x after announcement	-0.00361	0.00285	0.0242**	-0.0275**	0.00607	0.00546	-0.0204**
	(0.0553)	(0.00241)	(0.0113)	(0.0113)	(0.00414)	(0.00977)	(0.00961)
Constant	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES
Observations	29,276	29,472	28,363	22,581	14,653	14,101	11,217
R-squared	0.822	0.804	0.900	0.869	0.830	0.951	0.933

Notes: This table presents multivariate difference-in-difference estimates of the effect that the introduction of the Orderly Liquidation Authority had on overall bank risk, using pre- and post-treatment periods that stretch over 4 (columns (1) to (4)) or 2 (columns (5) to (7)) quarters. Panel A presents benchmark tests comparable to our baseline estimations, but with shorter pre- and post-treatment periods. Panel B tests the occurence of a gambling effect. *Affected bank* takes a value of 1 if the bank is part of a BHC with more than 30% of non-FDIA-regulated assets and a value of 0 if the bank is independent or part of a BHC with less than 10% of non-FDIA-regulated assets. *After OLA* is 1 for the quarters Q3 2010 - Q2 2011 (columns (1) to (4)) and Q3 2010 - Q4 2010 (columns (5) to (7)) and 0 for the quarters Q3 2008 - Q2 2009 (columns (1) to (4)) and Q1 2009 - Q2 2009 (columns (5) to (7)). *After announcement* is 1 for the quarters Q3 2009 - Q2 2010 (columns (1) to (4)) and Q3 2009 - Q4 2009 (columns (5) to (7)) and 0 for the quarters Q3 2008 - Q2 2009 (columns (1) to (4)) and Q1 2009 - Q2 2009 (columns (5) to (7)). Several dependent variables are tested: *z-score* (defined as mean return on assets plus capital ratio divided by the standard deviation of return on assets, only available for the 4-quarter period), *trading asset ratio* (defined as ratio of assets held in trading accounts to total assets), *low risk securities ratio* (defined as the ratio of securities of U.S. government agencies and subdivisions to total investment securities), and *high risk securities ratio* (defined as the ratio of equity securities, asset-backed securities, and trading accounts to total investment securities). Control variables comprise the natural logarithm of total bank assets, capital ratio, profitability, liquidity ratio, and an indicator variable that takes the value of 1 if the bank was a recipient of the TARP CPP program in a respective quarter (and 0 otherwise). All models include bank and time fixed effects.

Heteroskedasticity consistent standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

particularly in interaction with $afterOLA_t$. With regard to the definition of the treatment period and the pre- and post-treatment periods, we also use alternative variables, computed over 8, 6, and 4 quarters. Nevertheless, running our main bank risk-taking model with these alterations in the key explanatory variables yields results that are comparable in statistical and economic significance.

- In order to alleviate concerns about endogeneity in our model, we go beyond the univariate difference-in-difference approach and add bank and time fixed effects for regressions using the bank level dataset and bank and regional (tract level) fixed effects for regressions using the loan level dataset as well as sets of time-varying control variables (as appropriate). We tested all of our models in alternative specifications, including and excluding the controls and fixed effects.

- Where appropriate and mandated by theory, we use alternative model specifications. One important specification is the choice of regression model to test the application approval indicator, which is a binary variable. In Panel A of Table 7 we presents results using the Linear Probability Model (LPM) as estimation method. Although the LPM has serious drawbacks (i.e. heteroskedastic, can predict probabilities outside the range [0;1]), it can be appropriate in a panel-data setting (see Puri et al. (2011) for a detailed methodological discussion). We rerun these regressions with probit and logit models and obtain results that are consistent with the findings presented in Table 7.

- Like many other papers using a difference-in-difference methodology, we rely on a panel dataset with repeated cross sections of banks and several periods of data before and after the treatment. Bertrand et al. (2004) describe how this setup can be prone to autocorrelation problems that may lead to an underestimation of the standard errors. Therefore, we further correct standard errors for possible autocorrelation at the bank level (as suggested by Puri et al. (2011) and Wooldridge (2010)) and rerun our models. The results are comparable in size and significance to our findings in the baseline model.

- Finally, we address concerns related to our samples by correcting for outliers, restricting samples to explanatory variables consistent over time, and using entirely different levels of aggregation. First, there might be concerns that the results are driven by outliers, e.g. in the dependent variable or in the non-FDIA-regulated-share that is used to define the treatment variable. In the bank level dataset, we winsorize the dependent variable, the explanatory variable, and the control variables with one percent in their highest and lowest quantiles. We run all our tests using these winsorized versions of dependent, explanatory, and control variables, all together and each at once. All of our results are robust to these alterations and yield very similar outcomes. Second, to address concerns about consistency of key explanatory variables, we exclude banks that change status of $AFFECTED_i$ within our observation period. Our results do not alter when applying this restriction. Third, we tested our hypotheses on bank risk-taking for different levels of aggregation: BHC and bank level. Where possible according to data availability, we test and present both the bank and the BHC level results in parallel, which are largely identical in direction and significance.

Taken together, our robustness tests suggest that our main findings are not driven by variable definition, specification, or sample choice.

6 Concluding remarks and policy implications

In July 2010, the U.S. legislator enacted the Orderly Liquidation Authority as part of the financial system reform package, the Dodd-Frank Act. The OLA extends a special bank resolution procedure to financial institutions that were previously not covered by the provisions of the Federal Deposit Insurance Act, which allows the FDIC to resolve failed banks in an administrative procedure securing liquidity and discipline alike. Hence, the OLA affects financial institutions differently, raising the resolution threat particularly for those institutions that were in large part not subject to the FDIA resolution regime before.

Building on a recent theoretical model by DeYoung et al. (2013), we suggest several hypotheses how this regulatory change affects bank behavior, particularly risk-taking and business model choices. We propose a difference-in-difference framework exploiting the differential effect of the OLA to test these hypotheses. First and foremost, we find the results to be consistent with our main hypothesis: The introduction of the OLA changes the behavior of the affected financial institutions towards less risk-taking and safer business models as compared to the non-affected institutions. In the absense of treatment, i.e. of the regulatory change, both the affected and non-affected institutions behave equally, which further corroborates our results. Consistent with the theoretical prediction that the main effect varies with the credibility, capability, and the political will of the regulator to indeed resolve failed institutions, we find the effect to vanish for the largest, most systemically relevant institutions. Finally, we have to reject the hypothesis that affected banks gamble in between the announcement and the enactment of the OLA.

Our findings yield several interesting policy implications. If we consider our results an evaluation of a specific change in the U.S. bank resolution regime, we confirm that the Orderly Liquidation Authority is indeed an effective improvement to the regulatory arsenal. To the extent that a reduction in overall risk-taking of the previously non-FDIA-regulated financial institutions (as compared to their already regulated peers) was one of the legislator's intentions, our results suggest that it can indeed be considered successful. However, making OLA's resolution threat credible and thus effective for banks with the highest systemic importance while moderating the liquidity cost of winding down such institutions will remain a crucial challenge for the regulator.

Moreover, although our analyses focus on the effects of a specific resolution regime, i.e. the Orderly Liquidation Authority, our results induce us to also draw general implications for the design or reform of bank resolution regimes around the world. Based on these findings and previous literature, we propose three fundamental features of effective bank resolution regimes that - in our view - can help to increase and maintain stability in the financial system and prevent future financial crises. First, a bank resolution regime that takes into account the special role of financial institutions (contrary to the regular and often inapplicable corporate bankruptcy law) is essential, not only to avoid major interruptions in liquidity provision, but also to create a credible resolution threat for financial institutions in order to discipline them ex ante. Second, comprehensive coverage of financial institutions as a whole - that goes beyond the scope of deposit-taking entities only - will avoid incentives to shift risks into non-resolvable entities. Third, implementation speed is crucial. When the regulator succeeds in implementing the resolution threat quickly after its announcement, excessive gambling behavior in the lag time before enactment can be prevented.

Taken together, a bank resolution regime that incorporates these elements can be more than wishful thinking - it can be an effective threat that disciplines banks towards more prudent behavior.

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